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COMBINATION TONES AND OTHER RELATED AUDITORY PHENOMENA

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PREFACE.

The first part of this monograph is devoted primarily to a critical exposition of the important theories of combination tones and a statement of the facts upon which they rest. This undertaking inevitably leads to the mention of a considerable number of closely related phenomena whose significance for general theory is often crucial. In view of the conditions prevailing in the literature of the subject, it has been thought expedient that this presentation should in the main follow chronological lines. The full analytical table of contents, together with the division into sections, will readily enable readers who so desire to consult the text on special topics. The second part of the monograph reports certain experimental observations made by the author on summation tones.

In the pages which follow several physical terms are used which are commonly understood and need no definition here. A few may be new to readers not familiar with the literature of acoustics. A "pendular (or pendulum) vibration" is a simple, or sinus-form, vibration. In several quotations the word "note," and sometimes "sound," occurs where the meaning is precisely that of the word "tone." "Interruption tone" is used synonymously with "intermittent tone." By "transformation of the primaries" is meant the process of superposition of vibrations explained by Helmholtz in his mathematical determination of the origin of combination tones. A combination tone may, of course, be either a difference tone or a summation tone.

Where p and q are used to designate the two primary tones, p refers to the higher and q to the lower tone. Frequently h and t are used for the higher and lower tones respectively: these are most commonly used now, especially by German writers. N is sometimes used for the lower and n' for the higher tone. Some writers have used n for the interrupt*ed* and m for the interrupt*ion* tone; others have used these letters

in the reverse order. Since quotations are so frequently made from various writers no exclusive use of any of these letters can well be adopted here.

In designating the octave of any tone, usage also varies. Some writers use c^2, *e. g.*, where others use c'', and still others c_2. In this paper the now more common usage, . . . C_2, C_1, C, c, c^1, c^2, c^3 . . ., has been adopted, c being 128 d. vib. I have taken the liberty to change all other markings into this system in the quotations. In the *experimental* part, however, where the Koenig forks were used exclusively, I have simply employed the designations used by Koenig, *e. g.*, Ut_3, Re_3, etc., Ut_2 being the same as c (128 d. v.).

In quoting from the French and German where the translation is my own, I have used single quotation marks ('——').

The theories of Rutherford, Waller, Hurst, Emile ter Kuile, Ewald, and others, have not been considered in this paper. In the opinion of the writer, they have contributed nothing to the subject immediately under consideration, *i. e.*, certain *secondary* auditory phenomena, but are, as yet, concerned essentially with the *primary* phenomena of hearing.

It is a pleasure, in this connection, to express gratitude and to acknowledge obligation to my teachers, Professor J. R. Angell and Dr. J. B. Watson, from whom I have received constant advice and patient criticism. I am much indebted to Mr. W. V. D. Bingham for kind suggestions and assistance. I also wish to thank my fellow students of the psychological laboratory, who served as subjects for my experiments.

J. P.

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COMBINATION TONES AND OTHER RELATED AUDITORY PHENOMENA

PART I.

HISTORICAL AND CRITICAL.

SECTION 1. Early Observations and Theories, Combination Tones, Beats, Resonance Phenomena, Interruption Tones and so Forth.

It is well known that up to Helmholtz's day difference tones were supposed to originate from rapid beats. We shall see that this view had taken its rise naturally from the fact that the vibration-number of the noticed difference tone equals exactly the number of beats of the generating tones.

Combination tones were discovered in 1745 by Sorge,[1] who apparently heard, usually, only those which corresponded in pitch to what is now called the first difference tone. Krueger suggests[2] that Sorge heard this tone only in cases where it is reinforced by coincidence with one or more of the combination tones of higher order, as in the case of the fifth or the major third.

The pitch of the first combination tone was estimated an octave too high, by the Italian violinist, Tartini (1754).[3] This tone is sometimes called Tartini's tone. Its true pitch was determined theoretically by Chladni in 1802,[4] for intervals which may be represented by $n:n+1$. Wilhelm Weber

[1] A. Sorge, *Vorgemach der musikal. Composition*, 1745–47.

[2] 'Zur Theorie der Combinationstöne,' *Phil. Studien*, XVII., 1901, p. 207.

[3] Krueger, *op. cit.*, p. 189; also Helmholtz, *Sensations of Tone*, third ed. of Ellis' trans., 1895, p. 62 a.

[4] Chladni, *Akustik*, 1802, p. 207.

(1829)[5] also speculated on the pitch of this tone, and agreed with Chladni. He also remarked—what in many cases is true—that double clangs whose vibration ratio does not depart too much from the true relations (wahren Verhältnissen), *i. e.*, slightly mistuned consonances, should give the difference tone (weakened) of the nearest consonant interval.[6] Krueger thinks that the tone Weber had in mind is the intertone (Zwischenton) later discovered by Stumpf.[7]

Weber is led to the conclusion that when the vibration-ratio lies as near to 5:6 as it does to 4:5, it should be possible sometimes for one to hear a Tartini's tone a third lower than the second octave below the lower primary tone, and sometimes a tone two octaves below the lower primary. 'Investigations have failed thus far,' he said, 'to discover both of these.'[8] On the previous page he states somewhat definitely the view earlier announced by Thomas Young. 'When, *e. g.*, 4 vibrations of the one wave-series fall into the ear in the same period of time as 5 vibrations of the other, there will be experienced by the ear, in every such period, one increase (Anschwellen) and one decrease in intensity of each tone, or, when these recur too rapidly to be perceived separately, a deeper tone will be heard (empfunden), which is equal in pitch to the tone which would result if the drum were actuated by vibrations 4 times as slow (4 mal langsamer) as those of the first wave-series. Such a tone is two octaves lower than the one generated by the first wave-series' (p. 218).

In the meantime Baron Blein, whose work[9] was brought to Weber's notice by Alexander Von Humboldt, had been working with vibrating cords, using the fundamental tone c^1 (256) and the other primary tone varying from this pitch to 512 vibrations. He had heard a *second* combination tone with the interval 256:300 and with three other intervals between the

[5] W. Weber, 'Ueber die Tartini'sche Tönen,' Poggendorff's *Annal. der Physik*, XV., 1829, p. 216.

[6] Krueger, *op. cit.*, p. 191.

[7] C. Stumpf, *Tonpsychologie*, II., 1890, p. 480.

[8] W. Weber, *op. cit.*, p. 219.

[9] Le baron Blein, *Exposé de quelques expériences nouveaux sur l'acoustique*, etc., 1829.

fourth and the fifth.[10] Thomas Young had, however, long before (in 1800) made a similar discovery. He says in his report to the Royal Society on *Experiments and Enquiries Respecting Sound and Light:* "But, besides this primary harmonic (*i. e.*, Tartini's tone), a secondary note is sometimes heard when the intermediate compound vibrations occur at a certain interval, though interruptedly; for instance, in the coalescence of two sounds related to each other as 7:8, 5:7, or 4:5, there is a recurrence of a similar state of the joint motion, nearly at the interval of 5/15, 4/12, or 3/9 of the whole period; hence in the concord of a major third, the fourth below the key note is heard as distinctly as the double octave."[11] It is well known that the interval 4:5 (*e. g.*, $c^2:e^2$) gives, besides 1 (c), also the combination tone 3 (g^1) very distinctly.

Stimulated by the work of Weber, the Swedish acoustician, Hällström,[12] began his study of beats and combination tones, working on the violin and organ. He became convinced that the increasing beats of two diverging tones blend together, beyond a certain limit of frequency, into a continuous tone, and therefore, first established the general rule, 'that the first combination tone is determined by the difference of the vibration numbers of the primary tones.'[13] Hällström determined four orders of combination tones corresponding to the numbers $h - t$, $2t - h$, $2(h - t)$, and $3t - 2h$, where h stands for the upper and t for the lower primary tone. The third of these he heard, says Krueger,[14] only in three or four of the minor thirds, where this tone is equal in pitch to Krueger's D_4 (fourth combination tone), and once in the major third, where it is the same as D_3. Hällström observed beats of the lower primary tone with the first combination tone $h - t$, and therefore supposed that these two tones gave rise to the second combination tone, $t - (h - t) = 2t - h$. He thus obtained different *orders* of combination tones with respect to their

[10] Krueger, *op. cit.*, p. 191.

[11] *Works of Dr. Young*, by Peacock, Vol. I., 1855, p. 84.

[12] G. G. Hällström, 'Von dem Combinationstöne,' *Pogg. Annal.*, XXIV., 1832, 438 ff.

[13] Max Meyer, 'Ueber Kombinationstöne,' *Zeit. f. Psychol.*, XI., p. 178.

[14] Krueger, *op. cit.*, p. 193.

origin. Helmholtz erroneously adopted this idea[15] though it was inconsistent with his mathematical theory, as we shall see later.

Heinrich Scheibler, a silk manufacturer of Crefeld and the inventor of a tonometer, made a more careful study of beats, from resonated tones of tuning forks. His work is reported by the school teacher, Roeber.[16] 'He first conclusively established the fact that the number of beats of the mistuned (verstimmt) prime equals exactly the difference of the double vibrations of the generating tones. The point of fusion of beats into a unitary sensation he placed at 16 per second, without taking the pitch into consideration.'[17] Scheibler based his investigation 'on the results of Hällström and on the assumption that all beats, beyond the point of fusion, go over into combination tones.'[18]

But this view had long since been expressed by de le Grange,[19] and later by Thomas Young. In his report to the Royal Society, January, 1800, Young explicitly states the theory. "The greater the difference in pitch of two sounds," he writes, "the more rapid the beats, till at last, like the distinct puffs of air in the experiments already related they communicate the idea of a continued sound; and this is the fundamental harmonic described by Tartini."[20] This view has since been known as Young's theory.

This theory, though it later became widely accepted by acousticians, did not appeal to the physicist, G. S. Ohm. 'He had defined the tone physically as a sinus vibration.'[21]

According to this definition the ear, says Helmholtz, "*perceives pendular vibrations alone as simple tones, and resolves all other periodic motions of the air into a series of pendular*

[15] *Sensations of Tone,* p. 154 d.

[16] A. Roeber, 'Untersuchungen des Herrn Scheibler über d. sog. Schläge, Schwebungen, oder Stösse,' *Pogg. Annal.,* XXXII., 1834, p. 333 ff. and 492 ff.

[17] Krueger, *op. cit.,* p. 196. But see quotation from Meyer, above.

[18] *Ibid.,* p. 197.

[19] de le Grange, 'Nouvelles researches sur la nature et la propogation du son,' *Miscel. phil.-math. soc. priv.* Taurinensis, T. I., 1759. (I have not yet seen this article myself.)

[20] *Works of Dr. Young,* by Peacock, Vol. I., 1855, p. 84.

[21] Stumpf, *op. cit.,* p. 240; Ohm, *Pogg. Annal.;* Bd. 47 (1839), p. 513.

vibrations, hearing the series of simple tones which correspond with the simple vibrations."[22]

This is the so-called Ohm's law. It is expressed in this way by Lord Rayleigh: "It is found by experiment that, whenever according to theory a simple [*i. e.*, sinus-form, or pendular] vibration is present, the corresponding tone can be heard, but whenever the simple vibration is absent, then the tone cannot be heard. We are, therefore, justified in asserting that simple tones and vibrations of a circular type [*i. e.*, the simplest form of vibrations, sinus-vibrations] are indissolubly connected. This law was discovered by Ohm."[23]

Ohm's definition of a tone gave rise to the well-known discussion between Ohm and A. Seebeck.[24] Seebeck "was not always able to recognise upper partial tones, where Ohm's law required them to exist. In other cases where he did hear the theoretical upper partials, they were weaker than the theory required. He concluded that the definition of a simple tone as given by Ohm was too limited, and that not only pendular vibrations, but other vibrational forms, provided they were not too widely separated from the pendular, were capable of exciting in the ear the sensation of a single simple tone, which, however, had a variable quality. He consequently asserted that when a musical tone was compounded of several simple tones, part of the intensity of the upper constituent tones went to increase the intensity of the prime tone, with which it fused, and that at most a small remainder excited in the ear the sensation of an upper partial tone. He did not formulate any determinate law, assigning the vibrational forms which would give the impression of a compound tone."[25] Helmholtz took up the defense of Ohm. "The difficulty we experience in hearing upper partial tones," he writes, "is no reason for considering them to be weak; for this difficulty does not depend on their intensity but upon entirely different circumstances, which could not be properly estimated until the advances recently made in

[22] Helmholtz, *op. cit.*, p. 56.

[23] Rayleigh, *Theory of Sound*, I., 1894, p. 18.

[24] Ohm, *Pogg. Annal.*, LIX., 1843, 513 ff.; LXII., 1844, 1 ff. Seebeck, *Pogg. Annal.*, LX., 1843, 449 ff.; LXIII., 1844, 353 ff. and 368 ff.

[25] Helmholtz, *op. cit.*, pp. 58–9.

the physiology of the senses."[26] He says that for some of the best musical qualities of tone, the loudness of the first upper partials is not far inferior to that of a prime tone itself. "There is no difficulty in verifying this last fact by experiments on the tones of strings. Strike the string of a piano or monochord, and immediately touch one of its nodes for an instant with the finger; the constituent partial tones having this node will remain with unaltered loudness, and the rest will disappear."[27] In this way we can convince ourselves that the first and second upper partials are by no means weak. "For tones not produced on strings this *a posteriori* proof is not so easy to conduct, because we are not able to make the upper partials speak separately. But even then by means of a resonator we can appreciate the intensity of these upper partials by producing the corresponding note on the same or some other instrument until its loudness, when heard through the resonator, agrees with that of the former."[28] All this, it seems to me, is an argument merely to establish the fact that in the complex wave there are physical, or objective, constituents corresponding to upper partials.

Seebeck might well agree thus far with Helmholtz. His contention is that the *ear*, unlike a physical resonator, is unable to appreciate these partial physical vibrations as distinct, *i. e.*, to analyze them, except in a very imperfect manner. More to the point in dispute, then, Helmholtz points out that when a tone is first sounded alone, and the attention fixed upon it, it can be perceived even when the lower tone, with which it had on former occasions fused, appears. "In polyphonic music proper, where each part has its own distinct melody, a principal means of clearly separating the progression of each part has always consisted in making them proceed in different rhythms and on different divisions of the bars; or where this could not be done, or was at any rate only partly possible, as in four-part chorals, it is an old rule, contrived for this purpose, to let three parts, if possible, move by simple degrees of the scale, and let the fourth leap over several. The small amount of

[26] *Ibid.*, p. 58 c.
[27] *Ibid.*, p. 58 b.
[28] *Ibid.*, p. 58 c.

alteration in the pitch makes it easier for the listener to keep the identity of the several voices distinctly in mind."[29] In the case of compound tones where all the partials start together and continue with the same relative strength, and all cease at the same time, it is little wonder that an analysis into the various constituent sensations is more difficult. In such cases, even with a trained musical ear, it requires the application of a considerable amount of attention to make the analysis. The influence of the upper partials of a compound musical tone is, moreover, by no means unfelt. "They give the compound tone a brighter and higher effect. Simple tones are dull. When they are compared with compound tones of the same pitch, we are inclined to estimate the compound as belonging to a higher Octave than the simple tones. . . . It is very easy to make a mistake of an Octave. This has happened to the most celebrated musicians and acousticians. Thus it is well known that Tartini, who was celebrated as a violinist and theoretical musician, estimated all combinational tones an Octave too high.

"The problem to be solved, then, in distinguishing the partials of a compound tone is that of analysing a given aggregate of sensations into elements which no longer admit of analysis. We are accustomed in a large number of cases where sensations of different kinds or in different parts of the body, exist simultaneously, to recognise that they are distinct as soon as they are perceived, and to direct our attention at will to any one of them separately. Thus at any moment we can be separately conscious of what we see, of what we hear, of what we feel, and distinguish what we feel in a finger or in the great toe, whether pressure or gentle touch, or warmth. So also in the field of vision. Indeed, as I shall endeavor to show in what follows, we readily distinguish them individually from each other, and that this is an innate faculty of our minds. . . .

"The matter is very different when we set to work at investigating the more musical cases of perception, and at more completely understanding the conditions under which the abovementioned distinction can or cannot be made, as is the case in the physiology of the senses. We then become aware that two

[29] *Ibid.*, p. 59 d.

different kinds or grades must be distinguished in our becoming conscious of a sensation. The lower grade of this consciousness, is that where the influence of the sensation in question makes itself felt only in the conceptions we form of external things and processes, and assists in determining them. This can take place without our needing or indeed being able to ascertain to what particular part of our sensations we owe this or that relation of our perceptions. In this case we will say that the impression of the sensation in question is *perceived synthetically*. The second and higher grade is when we immediately distinguish the sensation in question as an existing part of the sum of the sensations excited in us. We will say then that the sensation is *perceived analytically*. The two cases must be carefully distinguished from each other.

" Seebeck and Ohm are agreed that the upper partials of a musical tone are perceived synthetically. This is acknowledged by Seebeck when he admits that their action on the ear changes the force or quality of the sound examined. The dispute turns upon whether in all cases they can be perceived analytically in their individual existence; that is whether the ear when unaided by resonators or other physical auxiliaries, which themselves alter the mass of musical sound heard by the observer, can by mere direction and intensity of attention distinguish whether, and if so in what force, the Octave, the Twelfth, etc., of the prime exists in the given musical sound."[30]

In an extended argument following the sentences quoted, Helmholtz points out that with respect to *all* our senses we perceive synthetically until by accident or by consciously directed experiment and attention the contents of our perceptions are further analyzed. He concludes, therefore, that the ear *does* actually take up the separate pendular vibrations of a clang, and that by practice and special training we may become able to perceive them as individual tone sensations, whereas without such practice they are unanalyzed.

It is generally agreed that physical resonators (such, *e. g.*, as the Koenig cylindrical resonators, the Helmholtz spherical resonators, tuning forks) take up only the pendular-form

[30] *Ibid.*, pp. 62 ff.

vibrations of the air, and each resonator only those vibrations, moreover, whose periods lie very near its own natural period. The more nearly perfect the resonator, the more closely must the period of the air wave correspond to its own in order to be taken up. Tuning forks, *e. g.*, respond only to frequencies very near their own. Helmholtz suggests an instructive experiment, in this connection, to show that a piano is able to analyze the vowels of the human voice into their pendular vibrations. " Raise the dampers of a pianoforte so that all the strings can vibrate freely, then sing the vowel *a* in *father, art,* loudly to any note of the piano, directing the voice to the sounding board; the sympathetic resonance of the strings directly re-echoes the same *a.* On singing *oe* in toe, the same *oe* is re-echoed. . . . The experiment does not succeed so well if the damper is removed only from the note on which the vowels are sung. The vowel character of the echo arises from the re-echoing of those upper partial tones which characterise the vowels. These, however, will echo better and more clearly when their corresponding higher strings are free and can vibrate sympathetically. In this case, then, in the last resort, the musical effect of the resonance is compounded of the tones of several strings, and several separate partial tones combine to produce a musical tone of a peculiar quality."[31]

This experiment may also well be used to illustrate Helmholtz's view of tonal analysis in the ear, if we think of each piano string as communicating with a nerve which mediates the corresponding tone sensation. The ear, like the piano, by means of the basilar membrane fibers, takes up the various constituent pendular vibrations of any complex wave. These constituent vibrations, then, call up the corresponding sensations, which, however, are usually perceived synthetically, except in case of special training and direction of attention. The resonance theory, though first clearly stated by Helmholtz, " was a conception that flitted before the minds of Thomas Young, Johannes Müller, and others."[32]

As has already been suggested, resonance may occur in

[31] *Ibid.*, p. 61 c.

[32] John G. McKendrick, in Schaefer's *Physiology,* Vol. II., 1900, p. 1179.

different degrees. "Thus a system may show free or forced vibrations. The period of a free vibration depends on the constitution of the system itself; the vibration is made by the system when disturbed from the position of equilibrium and left to itself. A forced vibration, on the other hand, has a period determined solely by the external force acting on the system. So long as the external force acts, the forced vibration continues, but a free vibration quickly dies away. Further, a vibrating system of one degree of freedom may have the amplitude of its movements reduced by damping. Damping will soon extinguish a free vibration, and its influence is felt on a forced vibration, when there is an approach to isochronism. Now, when a forced vibration is excited in any one part of a system, all the other parts are also influenced, and a vibration of the same period is excited, whose amplitude depends on the constitution of the system as a whole.[33] If a part of the system is especially affected within a certain limit of amplitude, it is in the position of a system having one degree of freedom acted on by a given force and independent of the natural period. Resonance usually occurs when there is an approximate equality of periods between the vibrating body and the resonator. In some cases, the amplitude within which resonance is possible may be considerable; in others, very small; and much depends as regards delicacy of resonance, on the degree of damping that may be called into play. . . . Tuning-forks are susceptible of sympathetic vibration to a remarkable degree, notwitstanding the difficulty of setting their mass in motion, because they admit of a long accumulation of minute impulses; but for this reason there must be precise agreement between the pitches of the two forks. It is also observed that, if a fork is thus set agoing, it continues sounding for a considerable time."[34]

Now if the analysis of tone is effected in the cochlea on the principle of resonance, we shall expect to find in that organ a rather complex differentiation of structure. Each constituent pendular vibration must be taken up by a certain part of the

[33] Cf. Rayleigh, *Theory of Sound,* Vol. I., revised edition, 1896, p. 70.

[34] McKendrick, *op. cit.,* p. 1177. On the principle of resonance, see also Lord Rayleigh, *Theory of Sound,* Vol. I., 1896.

analytic organ. Helmholtz and the followers of the resonance principle generally have repeatedly pointed out that no other theory accounts so well for the complexity which we actually find in the cochlea. Helmholtz also shows that the ear acts differently for different periods of frequency. 'Shakes' with ten interruptions to the second can be clearly and sharply executed on several instruments throughout almost the whole scale; yet from *A* downwards, in the great and contra octaves, they sound rough and begin to fuse or run together. On these instruments, however, the interruptions can be made just as sharply at the base end of the scale as at the other end, so this fusing together cannot be due to the mechanism of the instrument. "Now since the difficulty of shaking in the base is the same for all instruments, and for individual instruments is demonstrably independent of the manner in which the tones are produced, we are forced to conclude," says Helmholtz, "that the difficulty lies in the ear. We have, then, a plain indication that the vibrating parts within the ear are not damped with sufficient force and rapidity to allow of successfully effecting such a rapid alternation of tones.

"Nay more, this fact further proves *that there must be different parts of the ear which are set in vibration by tones of different pitch and which receive the sensation of these tones.*"[35]

Professor A. M. Mayer, in 1875, published the results of a more extended and quantitative study of this same phenomenon. He interrupted the tone of a tuning fork by rotating between it and the attuned resonator a disk with a certain number of perforations. "A rubber tube led from the nipple of the resonator to one ear, while the other ear was tightly closed with a lump of bees-wax. . . . On slowly rotating the disk I perceived a series of sharply separated explosions or beats. On gradually increasing the velocity of the disk these explosions gradually approached each other; and on reaching a certain frequency in their succession they blended into a continuous smooth sensation, similar to that experienced when the disk was removed and the fork vibrated gently before the

[35] *Ibid.*, p. 143 d.

resonator."[86] After this point of blending was reached a further increase in the velocity of the disk did not change the character of the sensation. "Extreme velocities, of course produce such violent agitations at the mouth of the resonator as to render experimenting impossible."

With the aid of two competent subjects Professor Mayer obtained results which he considered more reliable than those which he had previously published in the *American Journal of Science* for October, 1874. These results are succinctly given in the following table:[87]

S	N		D	L
Ut_1	64	1/25	.0395 sec.	2.5
Ut_2	128	1/45	.0222 sec.	2.8
Ut_3	256	1/70	.0142 sec.	3.6
Sol_3	384	1/102	.0098 sec.	3.7
Ut_4	512	1/130	.0076 sec.	3.9
Mi_4	640	1/152	.0065 sec.	4.1
Sol_4	768	1/166	.0060 sec.	4.6
Ut_5	1024	1/180	.0055 sec.	5.6

S represents the tone used; N, the number of double vibrations; D, the durations of the residual sensation, *i. e.*, the reciprocal of D is the number of interruptions per second required for a continuous tone. L is the number of wave-lengths contained in the separate impulses into which the sound had been divided in order to produce the continuous sensation; *e. g.*, $64 \div 25 = 2.5 +$.

From his results Mayer constructs a curve illustrative of the law which he seems to have established. "From the discussion of the curve of the experiments," he says, "we find that the law connecting the pitch of a sound with the duration of its residual sensation may be expressed thus,

$$D = \frac{3.2}{N + 31} + .002,$$

in which D equals, in fractions of a second, the duration of the residual sonorous sensation corresponding to N number of vibrations per second."[88]

[86] *Philosophical Magazine*, 4th Series, LIX., 1875, p. 353.

[87] *Ibid.*, p. 355.

[88] *Ibid.*, p. 356.

Helmholtz did not hold that the ear is a *perfect* resonator; sympathetic vibration in the cochlea is not of a very high order of sensitiveness, for, unlike tuning forks, the vibration soon subsides when the impinging wave ceases to act upon it; and, furthermore, individual parts of the basilar membrane or cochlea, though vibrating most easily to the periods of their own frequency, also respond less strongly to periods *near* their own. "This sympathetic vibration is still sensible for the interval of a semitone."[39] This is of course equivalent to saying that the vibration in the cochlea, answering to external vibrations, is to a degree forced and not wholly sympathetic. On this basis, then, he meets the objection urged later by several critics,[40] that so small structures are incapable of responding sympathetically to so slow vibrations as those of the lowest audible tones.[41]

That the ear can perceive such phase variations as are represented by the beating of two neighboring tones, is explained, not from the physical form of the complex wave, but from the fact that the areas of the basilar membrane affected by the two tones respectively overlap, so that each vibration series is periodically strengthened and weakened.[42] A thin string stretched on a sounding board on which are placed two vibrating tuning forks of nearly the same pitch, may be seen to vibrate in the same fashion. A stretched membrane, somewhat resembling the drum of the ear, may be made to carry a small stiff stylus. This stylus is made to draw the vibrations of a membrane on a rotating cylinder. When this membrane, then, is set into sympathetic vibration by two tones that beat, the undulating line drawn by the stylus shows that periods of strong vibration alternate with periods of almost entire rest. Similar curves have been made by Dr. Politzer, who attached the writing stylus to the auditory bone (the columella) of a duck, and then produced a beating tone by means of two organ pipes of

[39] *Sensations of Tone*, p. 144 c.

[40] *E. g.*, Hermann, *Archiv f. d. g. Physiologie*, LIX., 1891, p. 515; Max Meyer, *Zeitschr. f. Psychol.*, XVI., 1898, p. 20.

[41] Cf. Helmholtz, *op. cit.*, p. 146 d.

[42] *Ibid.*, p. 166; cf. article on 'Ebbinghaus' Explanation of Beats,' by Bentley and Titchener, *Amer. Jour. of Psychol.*, XV., p. 66.

nearly the same pitch. This experiment shows, says Helmholtz, that even the auditory bones follow the beats of two tones. " Generally this must always be the case when the pitches of two tones struck differ so little from each other and from that of the proper tone of the sympathetic body, that the latter can be put into sensible vibration by both tones at once. Sympathetic bodies which do not damp readily, such as tuning-forks, consequently require two exciting tones which differ extraordinarily little in pitch, in order to show visible beats, and the beats must, therefore, be very slow. For bodies readily damped, as membranes, strings, etc., the difference of the exciting tones may be greater, and consequently the beats may succeed each other more rapidly.

" This holds also for the elastic terminal formations of the auditory nerve fibers. Just as we have seen that there may be visible beats of the auditory ossicles, Corti's arches[43] may also be made to beat by two tones sufficiently near in pitch to set the same Corti's arches in sympathetic vibration at the same time. If then . . . the intensity of auditory sensation in the nerve fibers involved increases and decreases with the intensity of the elastic vibrations, the strength of the sensation must also increase and diminish in the same degree as the vibrations of the corresponding elastic appendages of the nerves. In this case also the motion of Corti's arches must still be considered as compounded of the motions which the two tones would have produced if they had acted separately. According as these motions are directed in the same or in opposite directions they will reinforce or enfeeble each other by (algebraical) addition. It is not till these motions excite sensation in the nerves that any deviation occurs from the law that each of the two tones and each of the two sensations of tones subsist side by side without disturbance."[44]

Now it becomes very evident that Helmholtz could not accept the theory of Thomas Young, that beats when they become rapid enough pass over into continuous tones. As the two

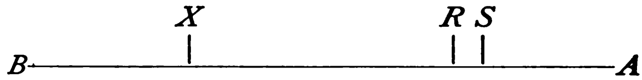
[43] While he speaks in terms of Corti's arches it is to be understood that, according to his later view, these only *mediate* the vibrations of the basilar membrane fibers. His argument, of course, applies equally well to these fibers.

[44] Helmholtz, *op. cit.*, p. 166 c.

primary tones diverge, the sections of the basilar membrane affected by these tones also separate farther and farther and finally cease altogether to overlap. The beats, consequently, dependent on this overlapping of vibrating sections, must gradually diminish in intensity as they increase in frequency, and at a certain point must 'run out.' With high tones more beats can be heard per second before this point of disappearance is reached than is the case with low tones, because high tones give more beats for a given separation, *e. g.*, a semi-tone, than do low tones. Beats, then, according to Helmholtz's view can occur only when their generating tones lie near together in pitch; but the term 'generating tones' here may mean primary tones, upper partials of the primaries, or combination tones. It is important to note that on Helmholtz's theory the *place of stimulation* in the cochlea, rather than the *manner* (*i. e.*, the frequency), is directly the determining condition of the pitch of the experienced tone.[45] It is on this very point that Ebbinghaus' view differs fundamentally from that of Helmholtz. If this important point in Helmholtz's theory is kept in mind, it is easy to understand why the view that beats go over into difference tones (Young) should be thrown out of court. The stimulation to which the first combination tone $(h - t)$ answers must be much farther up the cochlea than the stimulations corresponding to the primary tones, h and t. Think what an excursion the vibration for this combination tone would have to make over the basilar membrane fibers as the primary tones diverged! It might well be granted by Helmholtz, so far as I see, that two interfering pendular vibrations in the basilar membrane could give rise to pendular vibrations of other periods. A sufficient amount of asymmetry might well be found there. But these new vibrations would not give rise to new tone sensations, unless they started up, in the liquids of the cochlea, similar vibrations which could be taken up *by the fibers of that period.* To illustrate, suppose the tones c^4 and d^4, of 2,048 and 2,304 vibrations respectively, are given and that the sections of the basilar membrane thrown into

[45] *Periodicity* of course determines the *place* of stimulation, and so indirectly the pitch; cf. *Sensations of Tone*, p. 11.

sympathetic vibration by them overlap, to a degree. Now, on account of asymmetry in the membrane, new periods of vibration might be generated, *e. g.*, 256 (2,304–2,048). Suppose the sections of these stimulations are in the region represented on the accompanying line by *R* and *S*.



The new vibration series of 256 per second would of course also, then, be in this region of the membrane, whereas *the fibers sympathetic to that frequency* are located, say, in the region *X*, and the nerves corresponding to these fibers are the only ones which, when stimulated, will mediate the *sensation* of that tone.[46]

Now, the point made above is this: unless this new vibration-frequency can communicate its period to the surrounding fluid so that it can be taken up by the sympathetic fibers at *X*, no such tone will be sensed. And such a process is improbable.

This leads us naturally to Helmholtz's *positive* contribution, a new explanation of the origin of combination tones, the principle of which we have already anticipated. He had shown why on his own theory of cochlear action combination tones cannot result from rapid beats; now he was to explain how they *can* be produced, and in this very explanation he found, as we shall see, new reasons to believe that beats do not pass over into combination tones. " In the explanation of clang analysis and the operation of Ohm's law we have had to enunciate," says Helmholtz, " and constantly apply the proposition that oscillatory motions of the air and other elastic bodies, produced by several sources of sound acting simultaneously, are always the exact sum of the individual motions producible by each source separately. This law is of extreme importance in the theory of sound, because it reduces the consideration of compound cases to those of simple ones. But it must be ob-

[46] This supposition of the generation of new frequencies in the basilar membrane is only hypothetical, and is made solely for purposes of explanation. It illustrates clearly why on Helmholtz's theory one could not suppose that beats give rise to combination tones.

served that this law holds strictly only in the case where the vibrations in all parts of the mass of air and of the sonorous elastic bodies are of *infinitesimally small dimensions;* that is to say, only when the alterations of density of the elastic bodies are so small that they may be disregarded in comparison with the whole density of the same body; and in the same way, only when the displacements of the vibrating particles vanish as compared with the dimensions of the whole elastic body. Now certainly in all practical applications of this law to sonorous bodies, the vibrations are always *very small,* and near enough to being *infinitesimally small* for this law to hold with great exactness even for the real sonorous vibrations of musical tones, and by far the greater part of their phenomena can be deduced from that law in conformity with observation. Still, however, there are certain phenomena which result from the fact that this law does *not* hold with perfect exactness for vibrations of elastic bodies, which, though almost always *very small,* are far from being *infinitesimally small.* One of these phenomena, with which we are here interested, is the occurrence of *combinational tones.*"[47] "Now it may be shown that *combinational tones must arise whenever the vibrations are so large that the square of the displacements has a sensible influence on the motions.*"[48]

As a simple example Helmholtz develops mathematically the system of waves which would result from the motion of a single heavy point under the influence of two pendular wave series.

Let m represent the mass of a heavy point able to oscillate in the direction of the axis of X, and let the force which restores it to its position of equilibrium be

$$k = ax + bx^2$$

Now if we suppose that two systems of sonorous waves act upon it with the respective forces
$f \cdot \sin pt$ and $q \cdot \sin(qt + c)$, the equation of motion becomes

$$-m \cdot \frac{d^2x}{dt^2} = ax + bx^2 + f \cdot \sin pt + q \cdot \sin(qt + c).$$

[47] *Sensations of Tone,* p. 152.
[48] *Ibid.,* Appendix XII., p. 412.

From this equation Helmholtz obtains, besides the primary tones p and q, various combination tones and upper partials. Of these $2p$, $2q$, $p - q$, and $p + q$ may be considered to be of the first order; $3p$, $3q$, $2p + q$, $2p - q$, $p + 2q$, $p - 2q$, of the second order,[49] and so on. Of those of the first order the tone $(p - q)$ will have the greatest intensity, if the intensities of the primary tones are nearly the same; "the tone $(p + q)$ will be much weaker and the tones $2p$ and $2q$ will be heard with difficulty as weak harmonic upper partial tones." The intensity of the combination tones, according to this determination, must remain proportional to the product of the intensity of the generating tones; and hence must increase more rapidly than the primary tones.

"The previous assumption [which, of course, is a purely arbitrary one] respecting the magnitude of the force called into action, namely

$$k = ax + bx^2$$

implies that when X changes its sign, k changes not merely its sign, but also its absolute value. Hence this assumption can hold only for an elastic body which is unsymmetrically related to positive and negative displacements. It is only in such that the square of the displacement can affect the motion,[50] and combinational tones of the first order arise."

As is well known, Helmholtz supposed the unsymmetrical structure of the outer drum membrane of the ear to be a most favorable condition for the generation of combination tones.[51] "But a more important circumstance, as it appears to me, when the tones are powerful," he continues,[52] "is the loose formation of the joint between the hammer and anvil. If the handle of the hammer is driven inwards by the drumskin, the anvil and stirrup must follow the motion unconditionally. But

[49] This, as Helmholtz says, is according to Hällström's nomenclature. Hällström, it will be remembered, did not explain their *origin* in this way, however. The tones $p \pm 2q$, or $2q \pm p$ when the signs are changed, are the tones actually heard. Wherever these letters are used, p stands for the higher and q for the lower tone.

[50] Helmholtz, *op. cit.*, p. 413. The above determinations, constituting appendix XII., appeared in *Pogg. Annal.*, Bd. 99, 1856.

[51] *Sensations of Tone*, p. 158 b, 413 b.

[52] *Ibid.*, p. 158 b.

this is not the case for the subsequent outward motion of the handle of the hammer, during which the teeth of the two ossicles need not catch each other. In this case the ossicles may *click*. Now I seem to hear this clicking in my own ear whenever a very strong and deep tone is brought to bear upon it, even when, for example, it is produced by a tuning-fork held between the fingers, in which there is certainly nothing that can make any click at all."

Here, then, are two means by which, according to Helmholtz, combination tones may arise from two generating tones. They may be occasioned either (1) by the unsymmetrical vibration of some structure affected by both of the primary tones, or (2) by the peculiar action between the hammer and anvil of the ear. Combination tones originating in either of these ways have usually been called *subjective,* since they are generated within the ear itself. The term, of course, is a little misleading. These tones have never been reinforced by means of resonators, and have never been made to call forth sympathetic vibrations from the most delicate and carefully attuned forks.[53]

When, however, the primary tones are generated by such instruments as the polyphonic siren or the harmonium, *which have common wind supply for the generators of the two tones,* a third cause for combination tones exists. "I will here draw attention to a third case, where combinational tones may also arise from infinitely small vibrations.[54] . . . It occurs with sirens and hamoniums. We have here two openings, periodically altering in size and with a greater pressure of air on one side than on the other. Since we are dealing only with very small differences of pressure, we may assume that the mass of the escaping air is jointly proportional to the size of the opening ω, and to the difference of pressure p, so that

$$q = c\omega p$$

[53] Cf. Helmholtz, *op. cit.,* p. 153 c; W. Preyer, *Akustische Untersuchungen,* 1879 (Synopsis by Ellis in Helmholtz, *op. cit.,* pp. 531–2); Rücker and Edser, *Philosophical Magazine,* XXXIX., 1895, pp. 341–57; K. L. Schaefer, in Nagel's *Physiologie des Menschen,* III., 1905, p. 531.

[54] Would combination tones arise if the vibrations were *infinitely* small?

where c is some constant. If we now assume for ω the simplest periodic function which expresses an alternate shutting and opening, namely

$$\omega = A \cdot (I - \sin 2\pi nt),$$

and consider p to be constant, that is, suppose ω to be so small and the influx of air so copious, that the periodical loss through the opening does not essentially alter the pressure, q will be of the form

$$q = B \cdot (I - \sin 2\pi nt)$$

where

$$B = cAp.$$

" In this case the velocity of the motion of sound at any place of the space filled with air, must have a similar form, so that only a tone with the vibrational number n can arise. But if there is a second greater opening of variable size, through which there is sufficient escape of air to render the pressure p periodically variable,[55] instead of being constant, as the air passes out through the other opening, that is, if p is of the form

$$p = P \cdot (1 - \sin 2\pi mt)$$

then q will have the form

$$\begin{aligned} q &= cAP(1 - \sin 2\pi nt) \cdot (1 - \sin 2\pi mt) \\ &= cAP[1 - \sin 2\pi nt - \sin 2\pi mt \\ &\qquad - \tfrac{1}{2} \cos 2\pi(m + n)t + \tfrac{1}{2} \cos 2\pi(m - n)t]. \end{aligned}$$

Hence, in addition to the two primary tones n and m, there will be also the tones $m + n$ and $m - n$, that is, the two combinational tones of the first order.

" In reality the equations will always be much more complicated than those selected for showing the process in its simplest form. The tone n will influence the pressure p, as well as the tone m; even the combinational tones will alter p; and finally the magnitude of the opening may not be expressible by such a simple periodic function as we have selected for ω. This will occasion not merely the tones m, n, and $m + n$, $m - n$, to be produced, but also their upper partials, and the combina-

[55] This, then, seems to require 'vibrations' that are *finite*, and not 'infinitely small' as Helmholtz says above (see note immediately preceding this).

tional tones of these upper partials, as may also be observed in experiments. The complete theory of such a case becomes extraordinarily complicated, and hence the above account of a very simple case may suffice to show the nature of the process."[56]

Acousticians are now generally agreed that there are actually such tones as Helmholtz here determines theoretically. They are called *objective* combination tones because they have a cause external to the ear and may be reinforced by means of resonators. Helmholtz assures us that he himself heard them and has not only strenghtened them with resonators but has also made thin elastic membranes respond sympathetically to them.[57] He found, though, that even in cases where the primary tones were produced by instruments with a common wind-chest, "the greater part of the force of the combinational tone is generated in the ear itself. I arranged the portvents in the instrument [the harmonium]," he says, "so that one of the two generators was supplied with air by the bellows moved below by the foot, and the second generator was blown by the reserve bellows, which was first pumped full and then cut off by drawing out the so-called expression-stop, and I then found that the combinational tones were not much weaker than for the usual arrangement. But the objective portion which the resonators reinforce was much weaker."[58]

When the two generating tones have no mechanical connection, no reinforcement with resonators is possible In such cases the combination tones seem to have no corresponding *sensible* pendular vibrations in the air. They are, therefore, called 'subjective.'

For Helmholtz, however, 'subjective' is not a good term. All combination tones are 'objective' in the sense that they have corresponding pendular vibrations *external to the analyzing mechanism in the cochlea,* whether these vibrations take origin externally to the ear altogether or in the middle ear. 'It follows from the developments already given,' he says, 'that

[56] *Ibid.*, pp. 419–20, cf. also p. 157 a.

[57] *Ibid.*, p. 153 c, 157 b; especially *Pogg. Annal.*, CIX., 1856, p. 539.

[58] *Ibid.*, p. 157 c.

we do not necessarily have to seek the cause of combination tones in the manner of sensory reception (*Empfindungsweise*) by the auditory nerve, but that with two simultaneous tones of sufficient strength there can correspond to the combination tones actual vibrations of the drum and of the ossicles of the ear. These vibrations are experienced in the usual way by the nerve apparatus. Consequently the combination tones would not have a mere subjective existence, but would be objective, even though they can originate only in the vibrating parts of the ear.'[59] In certain cases we have found that their origin is entirely external to the ear.

'In the mathematical investigation of wave movements in the air,' continues Helmholtz, 'one as a rule concerns one's self only with the terms of the equations which contain the first power of displacements (Elongationen) of the air particles and one disregards the higher powers of such displacements. If the terms which contain the second power of displacements are retained, one finds the following: (1) Every point of the air mass, in which the vibrations corresponding to the individual primary tones are strong enough, becomes a center of new secondary wave systems, which correspond to the harmonical overtones of the respective [primary] tones. (2) Every point of the air mass where the vibrations of the two given primary tones simultaneously reach sufficient magnitudes (Grösse), becomes the center of new secondary wave systems which correspond to combination tones, and from these arise both difference and summation tones of the first and of higher orders.'[60]

Thus from purely theoretical considerations Helmholtz concluded that, besides the combination tones already discovered, there exist others with vibration-numbers corresponding to $h + t$, $2t + h$, etc. These tones he named *summation* tones, in contradistinction to those expressed by $h - t$, $2t - h$, etc., which he called *difference* tones.

Though summation tones are in most cases very weak, Helmholtz succeeded in hearing them and in thus verifying his theoretical determinations. Using the polyphonic siren, he

[59] 'Ueber Combinationstöne,' *Pogg. Annal.*, CIX., 1856, p. 537.

[60] *Ibid.*, p. 538.

heard not only the *first* summation tone but also those of the second order corresponding to $2p + q$, and $2q + p$ in his equation. The following table shows the summation tones of the second order which he heard:[61]

	Interval.	Sum. tone.
2:3	$(2 \times 2 + 3)$	7
2:3	$(2 \times 3 + 2)$	8
3:4	$(2 \times 3 + 4)$	10
5:6	$(2 \times 5 + 6)$	16
4:5	$(2 \times 5 + 4)$	14

Helmholtz thus, both theoretically and experimentally, obtains a third point to urge against the Young theory, that beats go over into combination tones. His three arguments against this theory, then, are these: (1) The theory does not explain the origin of summation tones. (2) Under certain conditions combination (both difference and summation) tones exist objectively, ' independently of the ear which would have had to gather the beats into a new tone.' (3) " This supposition [*i. e.*, Young's theory] cannot be reconciled with the law confirmed by all other experiments, that the only tones which the ear hears, correspond to pendular vibrations of the air."[62]

Before we consider the various objections that have been urged to these points, let us consider for a moment a certain inconsistency in Helmholtz's own statement of the origin of combination tones. " Multiple combinational tones [*i. e.*, those of higher orders] cannot in general be distinctly heard, except when the generating compound tones contain audible harmonic upper partials. Yet we cannot assert that the combinational tones are absent, where such partials are absent; but in that case they are so weak that the ear does not readily recognise them beside the loud generating tones and first differential. In the first place theory leads us to conclude that they do exist in a weak state, and in the next place the beats of impure intervals . . . also establish their existence. In this case we, as Hällström suggests,[63] consider the multiple com-

[61] *Pogg. Annal.*, CIX., p. 521.

[62] *Sensations of Tone*, p. 156 c; p. 167 ab. On the question of the universality of Ohm's law see below, pp. 43 ff.

[63] *Pogg. Annal.*, XXIV., p. 438.

binational tones to arise thus: the first differential tone, or *combinational tone of the first order*, by combination with the generating tones themselves, produces other differential tones, or *combinational tones of the second order;* these again produce new ones with the generators and differentials of the first order, and so on."[64]

This arrangement does not at all follow from Helmholtz's mathematical deductions given above, and is, moreover, in contradiction to well-known empirical facts to be considered later.[65] The tone $2q - p$, *e. g.*, deduced mathematically from the primary tones, p and q, under certain assumed conditions, is not formed by 'a combination of a difference tone of the first order with one of the primary tones,' according to the equation $q - (p - q) = 2q - p$; neither is it dependent upon a 'subjective' upper partial tone $2q$. Let us suppose that either one of these conditions of origin may be true and see where theoretically we shall land, even on Helmholtz's own assumption of 'transformation' due to asymmetry of the ear drum. Suppose that in the vibration of the drum to the tones p and q, the tones $2p$, $2q$, $p \pm q$ arise also in consequence of the asymmetry of that organ. Evidently neither $2q$ nor $p - q$ can react back on that *same* membrane so as to produce with one of the primary tones the combination tone $2q - p$ of the second order. Such a tone, therefore, must originate, if it does so at all, in some asymmetrical structure lying *still farther in*, where both the primary tones and the tones corresponding to $2q$ or $p - q$ will operate, in a sense, as coequals. But such a succession of origins in different structures is absurd when applied to the ear where probably only two successive structures of suitable asymmetry (the outer and inner membranes) exist. But, aside from our assumption, it seems plain that all the tones deduced by Helmholtz must spring directly from the effect of the primary tones; for $p - 2q$, or $2q - p$, *e. g.*, is just as much a product of the equation as is $2q$, or $p \pm q$.

This stand, supported by an argument quite different from

[64] Helmholtz, *op. cit.*, p. 154 d.

[65] Such facts, I mean, as second difference tones appearing much louder than the first.

the above, was taken in 1881 by R. H. M. Bosanquet in his article *On the Beats of Consonances of the Form h*: 1[66] (where '*h* is nearly some whole number'). Bosanquet shows mathematically, by means of what Ellis (who favors the 'beat-tone' view) calls some 'perhaps rather hazardous assumptions,'[67] that in the asymmetry of the drum "there are six summation-tones and six difference-tones produced by direct transformation of the primaries, when the effect of terms up to the fourth order [*i. e.*, to x_4 in the similar double integration process of Helmholtz's determination, p. 412, *Sensations of Tone*] is considered."[68] Bosanquet attempts "to show that those resultant sounds which depend on terms of higher orders can become great independently of those which depend on terms of lower orders."[69] He hopes thus to meet one of the chief objections urged against Helmholtz's theory, that the tones derived from terms of higher orders are in fact sometimes "produced with the greatest intensity when the tones derived from terms of lower orders are weak or evanescent." While it is not here maintained that Bosanquet has proved his point, the present writer is of the opinion that the true solution of this difficulty must be sought along this line; for even though the beat-tone theory, urged so eloquently by Koenig, be accepted, the difficulties are probably quite as great.

It is a question, of no less importance to the theory of combination tones than the point raised above, whether the two cases treated mathematically by Helmholtz are not *in principle* the same; that is, whether the deduction of objective combination tones from primaries generated with the siren or harmonium, *e. g.*, does not rest on the same principle ultimately as the deduction of such tones from a lack of symmetry in the forces of restitution of a heavy point vibrating under the influence of two primary tones. This question will be considered later.

Helmholtz had explained beats on the principle of *inter-*

[66] *Philosophical Magazine*, 5th Series, XI., 1881, pp. 420–36 and 492–506. Cf. on the question raised, p. 492–9.

[67] Helmholtz, *Sensations of Tone*, p. 532 d.

[68] Bosanquet, *op. cit.*, p. 497.

[69] *Ibid.*, p. 495.

ference in the basilar membrane. Such interference is possible only in case of small intervals. The beating of imperfect consonances he attributed to the presence of upper partials or of difference tones; in some cases, to both of these conditions.[70] He laid most stress on the presence of upper partial tones. Bosanquet, accepting this general point of view, early made an investigation of beats of various intervals.[71] Most of his experiments were made with tones from the enharmonic organ, in which he had a series of tones 'separated for the most part by single commas.' By means of tubes properly fitted into the ear and connecting with the resonator, he was able practically to shut out all vibrations of frequencies other than those under consideration.

After considerable practice he succeeded, as did also his friend, Mr. Parratt, '*in locating the beats with the lower tones of the intervals used.*' I quote his own words. "Suppose the mistuned octave $C:c$ was sounding, and I examined the lower note with the resonator; sometimes it appeared loud and steady, at other times as if beating powerfully. On removing the resonator-attachment from the ear, the lower note was always heard to beat powerfully. The explanation was simple. When the nipples of the resonator-attachment fitted tightly into the ears, nothing reached the ear but the uniform vibrations of the resonator sounding *C*. But if there was the slightest looseness between the nipple and the passage of either ear, the second note (*c*) of the combination got in, and gave rise to the subjective difference tone, . . . by interference of which with the *C* I explain the beats on that note. *These beats are therefore subjective.*"[72]

To avoid the introduction of 'all sorts of transformations depending on the greatness of the displacements,' Bosanquet used notes of moderate strength.[73] The notes employed were

[70] *Sensations of Tone*, p. 154 a, and especially ch. 10, p. 179 ff.

[71] R. H. M. Bosanquet, 'On the Beats of Consonances of The Form $h:l$,' *Phil. Mag.*, 5th Series, XI., 1881, p. 420 ff.

[72] *Ibid.*, p. 431.

[73] He criticises in this connection Koenig's experiments, soon to be considered. Koenig's tones were so loud that various combination tones might be introduced through transformation of the primaries on the principle explained by Helmholtz.

examined, with and without resonators, as to the presence of harmonics. These, so far as they are objective, are readily detected with resonators. The beats of the harmonics, where they existed objectively, were also examined with resonators. After a little practice the sound of these beats became familiar enough to prevent their being confused with the beats of the low notes, and the two sets of beats could be observed independently.[74]

Already Koenig had begun his excellent experiments which were reported in a series of articles,[75] later reprinted with some slight modifications in his *Quelques Expériences d'Acoustique* (1882). Koenig proves to be Helmholtz's most able opponent, and champions the cause of the theory that beats go over into combination tones, or beat-tones, as he designates them.

Koenig used powerful tuning forks actuated by an electric current and fixed before a large adjustable copper resonator. In order to vary the pitch of the forks he had the prongs hollowed out with a drill of small diameter. These borings were made to unite with each other at the stem of the fork and to communicate with a small reservoir of mercury. By raising or lowering the mercury with delicate adjustments the pitch of the fork could be lowered or raised correspondingly. This arrangement made possible an easy and gradual change in frequency of the fork within a comparatively wide range. For the lower tone of the interval he used a fork likewise resonated but not provided with the arrangement for changing the pitch.[76] "The forks that I used with resonators," says Koenig, "had no recognisable harmonic upper partials at all. The occurrence of harmonic upper partials in tuning-forks depends not so much on the lowness of their pitch and the amplitude of their

[74] *Ibid.*, pp. 427, 428. These results agree with Stumpf's and Meyer's introspections to be considered later. From my own experience I doubt very much that the two sorts of beats are as easily distinguishable as these men suppose. Of this more will be said later.

[75] The list is given by Ellis in his 'Additions' to Helmholtz's *Sensations of Tone*, pp. 527–8.

[76] R. Koenig, *Quelques Expériences d'Acoustique*, p. 87 ff.; Zahm, *Sound and Music*, 1892, pp. 315–6.

vibrations as on the relation of the amplitude to the thickness of the prongs."[77]

According to Koenig beats are produced not only in case of small intervals, as Helmholtz's theory requires, but are also heard, with less intensity, when the intervals are large. As the primary tones diverge from unison at *C* (64) the beats gradually increase in frequency. At the place where they cease to be perceived distinctly (about 12 or 13 per sec.) they begin to produce the effect of a rolling which is constantly accelerated until the interval reaches about the fourth (22 beats per sec.). Beyond the fourth to about the fifth there is a confused rumbling (ronflement), always very strong, which begins to clear up (débrouiller) as the interval approaches the sixth. Here one again hears a simple rolling, though very rapid. This rolling diminishes somewhere between the sixth and the seventh until, when the upper tone reaches the frequency of about 116 to 118 vibrations, one is again able to count 12 and 10 separate beats. These beats gradually grow slower until at the octave they disappear. These phenomena, Koenig goes on to explain, are the results of *two series of beats*. If we represent the interval by $n:n'$, the one set of beats, the louder ones, gradually increases from *O* to n, as the interval diverges from unison to the octave. These Koenig calls the *inferior*, or *lower* beats. At the same time the other series is decreasing in frequency from n to *O*. This series constitutes the *superior*, or *upper* beats. The confusion heard near the fifth is then easily explained; for the two series of beats cross, so to speak, at the frequency of $n/2$. At this place they exactly coincide. When the frequency of the inferior beats is much less than $n/2$ one hears these beats easily; when their frequency is considerably more than $n/2$ the superior beats are heard, less easily, however.

As n' rises above the octave, or $2n$, similar phenomena are experienced though with decreasing intensity. Under favorable conditions Koenig succeeded in hearing beats with intervals

[77] Koenig, Ueber den Ursprung der Stösse u. Stösstone u. s. w., *Weid. Annal.*, XII., 1881, p. 337, quoted from Ellis' translation in Helmholtz, *op. cit.*, p. 528.

as large as 1:9 ($C:d^2$) or 1:10 ($C:e^2$). These beats are of course very feeble.[78]

Now, according to Koenig's view, these beats, when they become rapid enough, are perceived as continuous tones. These he calls *beat-tones,* or beat-notes. Designating the lower beats or beat-tone by m and the upper by m', Koenig gives these formulæ for their determination:

$$m = n' - hn$$

and

$$m' = (h - 1)n - n',$$

where h is some whole number.[79] With the exception of a few cases, to be discussed later, the tones represented by m and m' in these equations are the only *primary* beat-tones that Koenig heard. *Secondary* beat-tones may, however, arise from the primary beat-tones as these have arisen from the generating tones.[80] Only in two cases did Koenig, according to his own account, hear secondary beat-tones, with the intervals 8:11 and 8:13 of the fourth accented octave. When $h = 1$, the tones represented by m and m' correspond to Helmholtz's first and second difference tones; in cases where $h = 2$, Koenig's lower beat-tone is the same as Helmholtz's second 'order' of difference tone (theoretical) corresponding to $p - 2q$;[81] and so on. Thus all Koenig's beat-tones find duplicates in Helmholtz's theoretical (at least) combination tones. Zahm says[82] that Helmholtz's theory does not explain the upper beat-notes of Koenig. This, at any rate, is not true of Helmholtz's mathematical theory, the only real explanation that he gave of combination tones. Zahm's mistake becomes evident if Helmholtz's deduction be carried a step further.[83] We shall later

[78] Koenig, *Quelques Expériences d'Acoustique,* p. 94.

[79] The terms 'lower' and 'upper,' as will be seen, have no reference to the *relative* pitches of the two tones. The *lower* beat-tone, as a matter of fact, often lies higher in the scale than the *upper.*

[80] This is denied by Krueger who has recently made a thorough study of combination tones and beats. *Phil. Studien,* XVII., 1901, p. 306.

[81] It will be remembered that Helmholtz's mathematical determination gave as combination tones of the second order $2p \pm q$ and $p \pm 2q$. Helmholtz himself heard the summation tones $p + 2q$ and $q + 2p$, cf. *supra,* p. 23.

[82] Zahm, *op. cit.,* p. 324.

[83] See below, p. 105.

consider more in detail the matter of combination tones. Suffice it here to say that any 'beat-note' which corresponds to a combination tone, as theoretically determined, need not trouble the followers of Helmholtz, *i. e.*, so far as the mere *existence* of such tone is concerned, aside from questions of audible intensity.

It would be wrong to infer that both the upper and the lower beats, or beat-tones, are always audible at the same time. This is in fact seldom the case. "If we divide the intervals examined into groups (1) from 1:1 to 1:2, (2) from 1:2 to 1:3, (3) from 1:3 to 1:4, (4) from 1:4 to 1:5, and so on, the lower beats and beat-tones extend over little more than the lower half of each group, and the upper beats and beat-tones over little more than the upper half. For a short distance in the middle of the period both sets of beats, or both beat-notes, are audible, and these beat-notes beat with each other."[84]

By means of a method for which Koenig is indebted to MM. Lissajous and Desains, he endeavors to make clear his idea of the origin of beats and beat-tones. Of two tuning forks he makes one carry on one of its prongs a smoked glass, and the other on one of its prongs a stylus, so placed that it can record on the glass the combined movement of the two forks. The forks are then actuated by an electric current. In this way a compound curve is constructed, the form of which is shown to vary according to the vibration ratio of the interval represented by the forks.[85] Koenig supposes that the ear experiences the beats—and, when these are fast enough, the beat-tones—directly from the form of the objective waves as represented by these curves. Here are his own words: "the beats of the harmonic intervals, as well as of the unison, should be deduced directly from the composition of waves of sound, and we should assume that they arise from the periodically alternating coincidences of similar maxima of the generating tones, and of the maxima with opposite signs. The similar maxima of these harmonic intervals, as in the case of unisons, will either exactly coincide, or else there will be maxima of condensation in the

[84] Ellis, in *Sensations of Tone*, pp. 529–30.

[85] Koenig, *op. cit.*, pp. 96–7.

higher tone lying between two successive vibrations of the fundamental tone, slightly preceding one and slightly following the other; but in both cases the effect on the ear will be the same, for a beat (fluctuation) is no instantaneous phenomenon, but arises from a gradual increase and diminution of the intensity of tone."[86] According to this view not only pendular vibrations, but any periodic intensity fluctuation within certain limits, may be sensed by the ear as tone. Koenig leaves to the physiologist the explanation of the actual process by which the ear accomplishes such analysis. He has, however, put forth a creditable number of experimental facts in support of his view. We proceed to examine some of them.

Among the first experiments on this subject which Koenig performed were those on periodically interrupted sounds. For this purpose he devised large brass disks with circles of holes of two cm. in diameter. He used three such disks with 16, 24 and 32 holes respectively. These disks when rotated before a tuning fork could be made to interrupt the tone at regular intervals. The interruptions, then, could very easily (by varying the rate of rotation) be increased or decreased in frequency. In some experiments the tuning-fork, not resonated, was held some distance away from the rotating disk and the tone was conducted to the circle of apertures in the disk through a tube whose diameter equaled that of the apertures. The observer was stationed at the opposite side of the disk.

Now Koenig found that when the tone of a tuning fork was interrupted in this way, a series of beats were heard, somewhat analogous to those of interference. When these beats reached a certain frequency they gave rise to a continuous tone. Thus when the tone c^2 (512 d. vib.) was interrupted 128 times per second there was heard not only the tone c^2 but also the 'interruption' tone c (128). But this was not all. Two other tones also appeared, with frequencies equal to the sum and the difference of the interruptions and the vibration number of the given tone. In the present instance these tones were $g'(512 - 128 = 384)$ and $e^2(512 + 128 = 640)$. These

[86] Ueber den Zusammenklang Zweier Töne, *Pogg. Annal.*, CLVII., p. 186. Quoted from Ellis' translation in *Sensations of Tone.*

tones, moreover, were so loud as to 'dominate' the feeble 'interruption' tone *c*. Koenig found that on substituting high tones for the given tone of the fork and keeping the rate of rotation constant, the tone *c* became increasingly distinct. When, *e. g.*, the fork Ut_6 (*i. e.*, c^4) and Ut_7, sounding very loudly, were used, so that the ratios of interruption to frequency of the given tone were respectively 1:16 and 1:32 the 'interruption' (or intermittence) tone acquired an extraordinary force, while the variation tones from the ratio 1:16 (*i. e.*, 15 and 17) were indistinct and those of the ratio 1:32 (*i. e.*, 31 and 33) were hardly perceptible.[87]

Koenig looks upon this experiment as a demonstration that pendular vibrations are not the only forms capable of being sensed as tone.

Koenig was by no means the first one, however, to perform experiments of this kind. The use of the rotating disk was preceded by other methods bearing on the same problem. One of the earliest of these methods was the rotation of a tuning-fork. As early as 1825, E. H. and W. Weber described in their *Wellenlehre* such an experiment: "If a tuning fork is put into a lathe so that it can be rotated about the longitudinal axis of its stem, it is found that the fork ceases to sound when a certain rate of rotation is reached, but that the tone reappears if the lathe is suddenly stopped. This is not to be explained by supposing that the noise of the lathe drowned the fork, for if one brings the end of a cylindrical tube close to the prongs of the fork and puts the other end to the ear, one is convinced that the rotation does not destroy the vibration of the fork, but prevents its transmission to the ear. We can give no explanation to this remarkable phenomenon."[88]

[87] *Quelques Expériences d'Acoustique*, pp. 138–9. If a tone *n* is interrupted *m* times, there result tones corresponding to n, m, $n-m$, and $n+m$. The tone corresponding to *m* is known as the 'interruption tone.' This name probably comes from Stefan (1866); cf. Bentley and Sabine, *Am. Jour. of Psych.*, XVI., note, p. 486. It is also called an intermittence tone. The tones $n-m$ and $n+m$ are known as 'variation tones' (from Radau), cf. p. 36, below.

[88] Quoted by W. Beetz, Über die Töne rotirender Stimmgabeln; *Pogg. Annal.*, CXXVIII, 1866, p. 490. Beetz's article was translated into English by G. C. Foster, *Phil. Mag.*, 4th Series, XXXII., 1866, 534 ff. I avail myself here of the statement by Bentley and Sabine of these early experiments, cf. *Amer. Jour. of Psychol.*, XVI., 487 ff.

In 1844 August Seebeck[89] theoretically investigates the matter of a tone m fluctuating periodically n times per second. He finds from mathematical deductions, that, according to Ohm's definition of a tone there should result from such fluctuation not only the tone m, but also tones corresponding to $m \pm n$, $m \pm 2n$, $m \pm 3n$, etc.[90] Seebeck did not follow up the matter experimentally. This, however, was done by Helmholtz who used for the purpose his double siren.

In 1863 Helmholtz describes his experiment in the first edition of his *Tonempfindungen*.[91] I quote from the translation of Ellis of a later edition: "The lower box of my double siren vibrates strongly in sympathy with the fork a^1 when it is held before the lower opening, and the holes are all covered, but not when the holes are open. On putting the disk of the siren in rotation so that the holes are alternately opened and covered, the resonance of the tuning-fork varies periodically. If n is the vibrational number of the fork, and m the number of times that a single hole in the box is opened, the strength of the resonance will be a periodic function of the time, and consequently in the simplest case equal to $1 - \sin 2mt$.

"Hence the vibrational motion of the air will be of the form

$$(1 - \sin 2\pi mt) \cdot \sin 2\pi nt = \sin 2\pi nt + \tfrac{1}{2} \cos 2\pi (m + n) t - \tfrac{1}{2} \cos 2\pi (m - n) t;$$

and consequently we hear the tones $m + n$, and $m - n$ or $n - m$. If the siren is rotated slowly, m will be very small, and these tones being all nearly the same, will beat. On rotating the disk rapidly, the ear separates them."[92]

In the meantime W. Beetz had been experimenting with the rotating tuning-fork. In 1851 he reported to the Physical Society at Berlin the results of a repetition of the Weber experiment mentioned above.[93]

In this experiment he had arrived at a result different

[89] 'Ueber die Definition des Tones,' *Pogg. Annal.*, LXIII., 1844, 353 ff.

[90] *Ibid.*, p. 366.

[91] *Lehre von den Tonempfindungen*, 1863, p. 597, cited by K. L. Schaefer in Nagel's *Physiologie*, III., 1905, p. 532.

[92] *Sensations of Tone*, p. 420.

[93] *Supra*, p. 32.

from that obtained by the Webers. For him the tone of the tuning-fork did not disappear; it only became weaker. "I distinctly heard at the same time," he says, "a higher tone as well as a series of beats which coincided in number with the number of half-revolutions of the tuning fork."[94] W. Weber, in a letter to Beetz, suggested 'that the reason why the higher note had not been perceptible in their [the Webers'] experiments was that they had employed a slower and more noisy lathe.' A satisfactory explanation was still wanting.

"Later Beetz again took up the experiment,[95] using two forks of 512 and 1024 vibrations per second. When these forks were rotated about twelve times per second, Beetz found that the pitch of the lower fork was raised about three-fourths of a tone, and the higher about a half tone. He heard again the beats, two for every revolution of the fork. The phenomenon, he holds, is not to be connected with the transmission of the vibrations of the fork to the air, for one hears the rise in the pitch just as well, or better, when one lays one's head on the lathe and stops one's ears entirely."[96]

"The phenomenon," says Beetz, "is thus entirely objective, and consists in a real increase of the rate of vibration of the fork. It is in fact only another form of Foucault's pendular-experiment. The vibrations tend to continue in the same plane as that in which they were produced; they are thus, as it were, transmitted to a thicker bar, and so produce a higher tone. The amplitude of the vibrations at the same time becomes smaller; gradually, however, it increases again, and reaches a minimum [Query, a maximum?—Trans. in a footnote] every time that the fork returns to its original position, or to one differing from it by 180°. It is thus that the beats are produced. . . . If the fork turns only slowly, the plane of vibration turns with it, and in this case the fundamental tone is heard above without beats; on turning more quickly, objective beats soon arise, and the tone

[94] *Phil. Mag.*, 4th Series, XXXII., 1866, p. 535.

[95] Über die Töne rotirender Stimmgabeln, *Pogg. Annal.*, CXXVIII., 1866, 490 ff. G. C. Foster translated this article into English, *Phil. Mag.*, 4th Series, XXXII., 534 ff.

[96] Quoted from Bentley and Sabine, *Am. Jour. of Psychol.*, XVI., p. 487; cf. Beetz, *Phil. Mag.*, 4th Series, XXXII., pp. 335–6.

rises at the same time, but never to an extent corresponding to the vibrations of a rod whose thickness is equal to the longer cross section of the prongs of the fork."[97] Later Beetz explains[98] that he discovered an error in his experiments which renders this explanation impossible. Furthermore, he has discovered certain lower tones which are apparently *not* objective and are not to be accounted for on the theory of the Foucault pendulum. He, therefore, attempts an explanation on the basis of the change in pitch of a moving source of sound. "The tone qualities present with a revolving fork could very well be explained in this way, but Beetz was entirely unable to make out any quantitative correspondence. The observed intervals were much too large. . . .[99]

"While Beetz was performing these experiments, J. Stefan was also carrying on investigations of a similar nature. He found that, if a vibrating plate were rotated before the ear, the characteristic tone of the plate disappeared and was replaced by two tones, the one higher, the other lower, than the primary. The higher is usually the stronger of the two, and the primary tone is sometimes audible along with the lower and higher tones. The same phenomenon is heard with a rotating tuning fork. The phenomenon to be explained, according to Stefan, is the effect upon the ear of a tone of periodically varying intensity. The movement which a tone of constant intensity produces in a body vibrating in sympathy with it can be expressed in the formula

$$a \sin 2\pi n (t + \theta)$$

where $n =$ vibration rate, t a variable, and θ a constant time, and a the amplitude of vibration. If the intensity varies periodically, a becomes a periodic function of t and in the simplest case can be expressed as

$$a \sin 2\pi n' (t + \theta') ;$$

n' being the number of intensity changes in a unit of time. a is

[97] Quoted from Foster's translation of Beetz, *op. cit.*, p. 536.

[98] 'Über die Töne rotirender Stimmgabeln,' *Pogg. Annal.*, CXXX., 1867, pp. 313–7.

[99] Bentley and Sabine, *op. cit.*, pp. 487–8.

then a constant quantity. If now one substitute this formula for a in the first, one gets for the excursion of the sympathetically vibrating body

$$a \sin 2\pi n'(t + \theta') \sin 2\pi n(t + \theta)$$

or

$$a/2 \cos \pi(n - n')(t + \theta_1) - a/2 \cos 2\pi(n + n')(t + \theta_2).$$

But each of these expressions represents a single pendular vibration, the one having a vibration rate of $n - n'$, and the other a rate of $n + n'$. By actual observation, Stefan found that his lower and higher tones corresponded in pitch to the demands of this explanation."[100]

In a second article which appears in the same periodical,[101] Stefan describes some experiments in which he interrupted the tone of a tuning-fork by the method already described,[102] which Koenig used later. We learn from this report that Professor Ernst Mach[103] had, at about the same time, also employed this method. Both investigators heard the tones corresponding to $m - n$ and $m + n$ of Helmholtz's equation, which Radau[104] had in the meantime determined theoretically. Radau calls these tones *variation tones*.

Beetz, stimulated by the results and the hypothesis of Stefan and Radau, takes up again the experiments with a view to testing this hypothesis.[105] "Finding the hypothesis well borne out with rotating plates, he turned again to rotating forks to determine whether they too gave the tones to be expected from Stefan's formula. He found that the lower tone as observed corresponded approximately to the calculated tone. The higher tone, however, was always much higher than the theory required. The difference between the observed and calculated

[100] Bentley and Sabine, *op. cit.*, p. 488; cf. also 'Über einen akustischen Versuch,' *Sitzungsber. d. kais. Akad. d. Wiss. zu Wien, Math.-naturwiss. Kl.*, LIII., Abt. 2, 1866, 696 ff.

[101] *Ibid.*, LIV., Abt. 2, 1866, 597 ff.

[102] *Supra*, p. 31.

[103] 'Ueber die Aenderung des Tones und der Farbe durch Bewegung,' *Pogg. Annal.*, CXII., 1861, 58 ff.

[104] *Moniteur Scientifique*, 1865, 430 ff.

[105] 'Ueber den Einfluss der Bewegung der Tonquelle auf die Tonhöhe,' *Pogg. Annal.*, CXXX., 1867, 587 ff.

values became very large with rapid rates of rotation. Beetz found, however, that the difference became trifling when he took his observations with a resonator having an opening 5 mm. instead of 25 mm. in diameter. With such a resonator the observed values coincided very closely with the values computed by Stefan's formula. Beetz used these forks, $c^1 = 256$, $a^1 = 440$, $c^2 = 512$, and three rates, 6.5, 13 and 19.5 revolutions per second. He took, also, some observations for two very low forks, 64 and 77 vibrations. In almost every case, Beetz's observed values are larger than the calculated values. In this last paper, Beetz accepted Stefan's and Radau's explanation of the phenomenon."[106]

All these studies have been made by men interested in physics, primarily, and they have treated these *variation tones* as actually existing *objectively as pendular vibrations.* Stefan and Beetz had actually used resonators in determining their pitch. Moreover, while explanations of the phenomenon at first differed considerably, or were entirely withheld, one cannot help being struck with the steady advance toward uniformity of opinion as theoretical determinations go hand in hand with the accumulation of empirical data. The outcome of the whole process is a brilliant achievement. Yet in the face of all this, Koenig, also a physicist, considered the *interruption,* or intermittence tone, an objection to the resonance hypothesis, without, apparently, testing for its objectivity![107]

In 1875 Professor A. M. Mayer, engaged on another problem, but using the same sort of interruption apparatus as Koenig employed, incidentally noticed this phenomenon of resultant tones and describes it practically as did Koenig. "When the disk is stationary, with one of its openings opposite the mouth of the resonator, it is evident that the ear will experience a simple sonorous sensation when a tuning-fork is brought near the mouth of the resonator. On revolving the perforated disk, two additional or secondary tones appear,—one slightly above,

[106] Bentley and Sabine, *op. cit.*, p. 489.

[107] None of these formulæ, it is true, show a vibration corresponding to this tone *m*, whose frequency number is equal to the number of interruptions; but we shall see that two explanations may be given of its origin, neither of which is contradictory to the resonance hypothesis.

the other slightly below the pitch of the fork. An increase in velocity of rotation causes the two secondary sounds to diverge yet further from the note of the beating fork, until, on reaching a certain velocity, the two secondary sounds become separated from each other by a major sixth, while at the same moment a resultant sound appears, formed by a union of the fork with the upper and lower of the secondary sounds. The resultant is the lower second octave of the note given by the fork.[108] On further increasing the velocity of rotation of the disk, the two secondary sounds and the resultant disappear, and the ear experiences only the sensation of the simple sound produced by the fork, whose beats at this stage of the experiment have blended into a smooth continuous sensation."[109]

Mayer's statement, at the conclusion of his article, that the beats from this rotating disk in the interruption of a continuous sound are like those due to the interference of two tones nearly equal in pitch, calls out from Lord Rayleigh the following: "The difference between the two kinds of beats is considerable. If there are two vibrations of equal amplitude and slightly differing in frequencies, represented by $\cos 2\pi n_1 t$ and $\cos 2\pi n_2 t$, the resultant may be expressed by

$$2 \cos \pi (n_1 - n_2) t \cos \pi (n_1 + n_2) t,$$

and may be regarded as a vibration of frequency $\frac{1}{2}(n_1 + n_2)$, and of amplitude $2 \cos \pi (n_1 - n_2) t$. Hence, in passing through zero, the amplitude changes sign, which is equivalent to a *change of phase of* 180°, if the amplitude be regarded as always positive. This change of phase is readily detected by measurements in drawings traced by machines for compounding observations. If a force of the above character act upon a system whose natural frequency is $\frac{1}{2}(n_1 + n_2)$, the effect produced is comparatively small. If the system start from rest, the successive impulses coöperate at first, but after a time the later impulses begin to destroy the effect of former ones. The greatest response is given to forces of frequency n_1 and n_2, and not of a force of frequency $\frac{1}{2}(n_1 + n_2)$.

[108] This is probably the so-called interruption tone. He seems to regard this tone as a difference tone. Cf. Schaefer's view, p. 86, below.

[109] *Phil. Mag.*, 4th Series, LIX., 1876, p. 358.

"On the other hand, where a single vibration is rendered intermittent by the periodic interposition of an obstacle, there is no such change of phase in consecutive revivals. If a force of this character act upon an isochronous system, the effect is indeed less than if there were no intermittence; but as all the impulses operate in the same sense without any antagonism, the response is powerful. An intermittent vibration or force may be represented by

$$2(1 + \cos 2\pi mt) \cos 2\pi nt,$$

in which n is the frequency of the vibration, and m the frequency of the intermittence. The amplitude is always positive, and varies between the values 0 and 4. By ordinary trigonometrical transformation the above expression may be put into the form

$$2 \cos 2\pi nt + \cos 2\pi(n + m)t + \cos 2\pi(n - m)t;$$

which shows that the intermittent vibration is equivalent to *three* simple vibrations of frequencies n, $n - m$, and $n + m$. This is the explanation of the secondary sounds observed by Mayer."[110]

From tones that are periodically interrupted Koenig went to periodically variable tones. In this experiment Koenig hoped by a sort of synthetic process to produce beats similar to those due to interference of neighboring tones and by increasing their frequency to prove that they produce a continuous tone. He used a large disk with seven circles of 192 holes each. The holes of each circle varied periodically in diameter from 1 to 6 mm. The seven circles of holes were arranged to vary periodically from the outside inward 12, 16, 24, 32, 48, 64 and 96 times respectively. In blowing against these circles through a tube 6 mm. in diameter when the disk was in rotation, Koenig heard not only the tone corresponding to 192—the number of holes to the revolution—but also, when the rotation was fast enough, the one in each case corresponding to the number of variations in diameter—*i. e.*, 12, 16, 24, etc., as the case may be. When the rotation was slow he heard a beating; as it

[110] *Phil. Mag.*, IX., 1880, pp. 278–9; cf. also Rayleigh, *Theory of Sound*, Vol. I., 1894, pp. 71–2.

increased, the beats became more and more rapid until they finally blended into a second pure tone.[111]

Zahm says that 'the foregoing experiment would seem to be conclusive as to the true nature of beats and beat-tones.'[112] Koenig was not so sure of this, however. He recognized that if the complex vibration thus produced were actually like that resulting from two interfering tones, we should expect to hear in this case not only the 'beat-tone' but two other tones corresponding to two primary tones which would produce similar beating. To quote: 'In short, if a series of 96 isochronous impulses of which the intensity increases and decreases 16 times represent exactly two tones which give 16 beats, one ought to hear the two primary tones in question (which would here be the tones 88 and 104, forming the interval 11:13); but these tones are never produced. The reason for this difference between the beats and the separate isochronous impulses of periodic intensity ought to be sought in the fact that the compound of two tones a and b near unison is equal to a tone of medium pitch $(a+b)/2$ of which the intensity not only increases and diminishes periodically but undergoes a *change of sign* once during each beat, as the formula

$$\sin a + \sin b = 2 \cos \frac{a-b}{2} \cdot \sin \frac{a+b}{2}$$

shows, where $\cos (a-b)/2$ represents the periodic intensity of the tone $(a+b)/2$.'[113]

When Mr. Spottiswoode reported Koenig's experiment,[114] Lord Rayleigh did not consider it at all convincing against Helmholtz's theory, *i. e.*, that beats do *not* generate combination tones. He made the same distinction between these 'beats' and those due to interference that Koenig did, consequently he could not see how beats of *interference*, with this change of phase,[115] could generate a tone sensation. Bosanquet raised the

[111] Koenig, *op. cit.*, pp. 140–142, and *Pogg. Annal.*, CLVII., 1876, pp. 177 ff.; cf. also Zahm, *op. cit.*, pp. 334–5.

[112] Zahm, *op. cit.*, p. 335.

[113] Koenig, *op. cit.*, p. 143.

[114] *Proc. Mus. Ass.*, 1878–9, p. 128.

[115] *Supra*, p. 38.

same objection, and developed it more fully in his article referred to above.[116] He shows, by a mathematical expression similar to that developed by Koenig above, that the 'resultant displacement'[117] should produce a tone "having oscillations of intensity whose frequency is defined by a pendulum-vibration of frequency equal to half the difference of the frequencies of the primaries. This," he says, "is what is actually heard in case of two notes less than two commas apart." If this held for widely separated intervals "the primary notes would not be heard at all, and the note that would be heard would have the arithmetic mean of the frequencies of the primaries.

"*E. g.*, in the case of a fifth (4:6) the note heard would be the major third (5), which would beat very rapidly. . . . But as a matter of fact, the note 5 is not heard at all in the above case." Again, 'supposing that in some unexplained way the beats whose speed is $(p - q)/2$ [*i. e.*, half the difference of the frequencies of the primaries] . . . gave rise to a note, as supposed by Koenig. Then the speed of that note does not agree with that required for Koenig's first beat-note, which has the same speed as Helmholtz's difference-tone, or $(p - q)$.'

Koenig seems fully to have appreciated the force of these objections, for he immediately takes up experiments to meet them.[118] "If two tones of 80 and 96 d. vib. are sounded together," he says, "they generate a tone of ½(80 + 96) = 88 vibrations with an intensity increasing and diminishing 16 times, and at each passage from one beat to another there is a change of sign, so that the maximum of compression of the first vibration of the following beat is half a vibration behind the maximum compression of the last vibration of the preceding beat."[119] To meet this case he performed two experiments.

[116] *Phil. Mag.*, 5th Series, XI., 421–3.

[117] Where the two vibrations $\cos pt$ and $\cos(pt - e)$, having equal amplitudes (= 1), are combined on the same receptive mechanism,

$$2 \cos \frac{(p+q)t - e}{2} \cdot \frac{(p-q)t + e}{2}$$

is the 'resultant displacement.'

[118] Koenig, 'Ueber die Zusammenklang zweier Töne,' *Pogg. Annal.*, CLVII., 1876, pp. 177–237.

[119] *Ibid.*, p. 252, quoted from Ellis' translation in *Sensation of Tone*, p. 534.

In the first he divided a circle on a large disk into 176 equal parts, numbering the parts in their order. In the five points 1, 3, 5, 7, 9, he drilled five holes, gradually increasing in diameter to 5 mm. and then diminishing. He did likewise in the points 12, 14, 16, 18 and 20; and in the points 23, 25, 27, 29 and 31; and so on. "When such a disk was blown upon through a pipe with the diameter of the largest opening, in addition to the tone 88 and the very powerful tones of the period 16 both of the tones 80 and 96 could be heard, but they were very weak, and, on account of the roughness of the deep tone, difficult to observe." 'In this case the phase was the same throughout,' says Ellis.[120] Koenig, too, apparently considered it so.

To imitate the *change of phase* Koenig divided each of two concentric circles on a disk into 88 parts and disposed the holes which were to represent the successive beats alternately on each circle. As 88 holes and 16 periods give 5½ holes to each period, Koenig 'took two periods together, and pierced on the first circle the divisional points 1, 2, 3, 4, 5, 6 and on the second 6, 7, 8, 9, 10, 11, then again on the first 12, 13, 14, 15, 16, 17 and on the second 17, 18, 19, 20, 21, 22, and so forth.'[121] The size of the holes, in this case also, gradually increased and diminished to represent beats. 'When these circles of holes were blown upon at the same time through two pipes of the diameter of the largest opening, and placed on the same radius, one circle from above and the other circle from below, then at each revolution of the disk there were created 88 isochronous impulses, varying 16 times in intensity, which changed sign on each transmission from one period of intensity to the other. In this experiment the two tones 80 and 96 were more distinct than in the first experiment, where the circles of holes were blown upon from one side only.'[122]

In reply to Bosanquet's objection,[123] that Koenig's beat-note

[120] Lord Rayleigh says: 'In passing through zero, the amplitude changes sign, which is equivalent to a *change of phase of* 180°, if the amplitude be always regarded as positive,' *supra*, p. 38.

[121] Cf. Fig. 41, *Quelques Expériences d'Acoustique*, p. 144.

[122] *Pogg. Annal.*, CLVII., pp. 232–3; cf. also *Quelques Expériences d'Acoustique* (where the two kinds of disks are shown in Figs. 40 and 41), pp. 144–5.

[123] *Supra*, pp. 40, 41.

ought not to agree in pitch with Helmholtz's first difference tone, Koenig in his later French edition[124] says: 'the change of phase of the separate vibrations of a variable amplitude, forming the beats, does not cause these maxima of intensity to be produced in contrary directions. Besides, these maxima remain isochronous, and consequently fulfill the conditions under which primary impulses are combined to form sounds. The only influence which the change of phase in question exerts on the disposition of the waves consists in the fact that these maxima of intensity do not stand apart by a whole number of complete vibrations $((a+b)/2)$, but by an odd number of half-vibrations. . . . Notwithstanding the change of phase, the beat-note must always have the same frequency as the beats.'

Koenig has further studied the influence of phase separate from the phenomena of beats. For this he employed his well-known wave-siren. "He first drew to scale the curves obtained by compounding partials up to the tenth number of the series; and he modified the curves so that they were compounded first with zero difference of phase, then with all the upper members moved one quarter, then with a difference of half a wave, and lastly with a difference of three quarters. The sounds of all these curves, according to Helmholtz, should be exactly alike, although they differed in form and position." These curves were reproduced accurately in the proper size by photography, inverted so that the high parts became low and the low high and then were cut out on the circumference of metallic cylindrical hoops. "These hoops were then mounted on an axis and rotated rapidly. Against these toothed edges (or edges showing sinusoidal curves) air was blown under pressure through narrow slits as the curves passed in front of the slits; and thus sounds varying in phase from a quarter to three-fourths of the wave-length were obtained. It was found that they *varied in quality*."[125] Koenig's own statement of the results follows: "The composition of a number of harmonic

[124] *Quelques Expériences d'Acoustique*, p. 143, note.

[125] McKendrick, in Schaefer's *Physiology*, Vol. 2 (1900), p. 1176; cf. Koenig, Bemerkungen über die Klangfarbe, *Wied. Annal.*, XIV., 1881, pp. 369–93; and especially *Quelques Expériences d'Acoustique*, pp. 222 ff., where there are illustrative figures of the curves and apparatus.

tones, including both the evenly and unevenly numbered partials, generates in all cases, quite independently of the relative intensity of these tones, the strongest and acutest quality tone for the 1/4 difference of phase, while the difference 0 and 1/2 lie between the others, both as regards intensity and acuteness.

"When unevenly numbered partials only are compounded, the differences of phase 1/4 and 3/4 give the same quality of tone, as do also the differences 0 and 1/2; but the former is stronger and acuter than the latter.

"Hence, although the quality of tone principally depends on the number and relative intensity of the harmonic tones compounded, the influence of difference of tone [phase?] is not by any means so insignificant as to be entirely negligible. We may say, in general terms, that the differences in the number and relative intensity of the harmonic tones compounded produces those differences in the quality of tone which are remarked in musical instruments of different families, or in the human voice uttering different vowels. But the alteration of phase between these harmonic tones can excite at least such differences of quality of tone as are observed in musical instruments of the same family, or in different voices singing the same vowel."[126]

"A ready appreciation of such minor differences," says Lord Rayleigh, "requires a series of notes, upon which a melody can be executed, and they may escape observation when only a single note is available. To me it appears that these results are in harmony with the view that would ascribe the departure from Ohm's law, involved in any recognition of phase relations, to secondary causes."[127] McKendrick thinks, from the results of this experiment, that the influence of phase is, however, not so absolutely negative as Helmholtz supposed.[128] Undoubtedly Koenig's method, ingenious as it is, would not allow of the reproduction of the tone exactly corresponding to the compound wave form. Any conclusion, therefore, drawn from the experiment described must take this fact into consideration.

Hermann, who adopts the view that beats run into com-

[126] *Wied. Annal.*, p. 391, trans. by Ellis in Helmholtz, *op. cit.*, p. 537.

[127] Rayleigh, *op. cit.*, Vol. II., p. 469.

[128] Schaefer's *Physiology*, Vol. II., p. 1176.

bination tones or beat-tones, holds that Koenig's experiment with the wave siren is inadequate as a proof that phase change of upper partials has an effect on the quality of the tone. The compressibility of the air, he holds, makes it altogether improbable that the air vibrations take the exact form of the curve on the wave siren.[129] He considers the siren, though a useful apparatus for other experiments, entirely unfit for use on the phase problem. He, therefore, took up experiments with Edison's phonograph,[130] and came to the conclusion that not the phase change in itself but the effect that is produced by such shift of upper partials on the position of the maxima in the compound curve and on the amplitude of vibration accounts for the change in the sound.[131] Hermann had already concluded that, in order to explain certain phenomena of beats and combination-tones, we must '*ascribe to the ear the power of answering with a tone sensation to every sort of periodicity within certain limits.*'[132] According to this supposition the question of phase needs to be formulated differently. It is best to let the word phase drop altogether and simply to say that the tone is conditioned not only by the amplitude of the partial vibrations but also, and chiefly, by the form (Gestalt) of the resulting curve. Hermann finds that, other things being equal, *the tone is always 'sharpest' when the change from maximum to minimum* (or vice versa) *is most abrupt.* So, at least, I understand him.[133] In any case, Helmholtz's theory seems to be insufficient on this point.

To recur then to the experiments of Koenig, we may well say that the results he obtained were in no way conclusive. That there is such a thing as an 'interruption' tone is by no means established. Physical theory shows that the so-called variation tones exist objectively both when they are produced

[129] Pflüger's *Archiv f. d. gesammte Physiol.*, LVI., 1894, p. 474.

[130] *Ibid.*, LIII., 1892, 8 f.; also LVI., 476 ff.

[131] *Ibid.*, LVI., p. 484.

[132] *Ibid.*, LIX., 1891, p. 514.

[133] *Ibid.*, LVI., p. 473. "*Der Schall ist unter sonst gleichen Umständen am shärfsten, wenn die Spaltöffnung durch die Curve so plötzlich als möglich verdeckt oder freigelegt wird, und zwar wirkt plötzliche Verdeckung weit schärfer als plötzliche Oeffnung.*"

as a consequence of periodic intermittence of a constant tone, and when they result from such intensity changes as are produced by the rotation of a sounding tuning-fork. Indeed both of these cases, as physical theory shows, may be treated together. From the experimental results of Koenig it is not so certain that the case of periodic intensity fluctuation produced by perforations of varying diameter may be classed with the other cases of periodic intensity changes or intermittence; for in this case the variation tones are not mentioned, and the ' interruption ' tone is very prominent.[134] Koenig has, however, by no means proved that beats of interference of neighboring tones produce beat-tones, even though it be granted that the beats of intensity variation are capable, when their frequency is sufficient, of producing a continued tone-sensation. Mathematical theory shows that in the case of beats from interference there is a *change of phase*. Even though Koenig, by an ingenious method, has endeavored to imitate this change of phase in case of the beats from intensity fluctuation, and under those conditions has still obtained the so-called interruption tone, it is not at all certain that he actually obtained the conditions that he sought. It is still an open question whether the ear perceives intensity fluctuations occurring with changes of phase.[135]

[134] But cf. K. L. Schaefer's experiments reported below, p. 88.

[135] Since the above account was written some recent articles on the perception of phase-difference have appeared in the *Philosophical Magazine*. Lord Rayleigh in No. 74 (February, 1907) reports experiments in which two tones of nearly equal pitch were conducted separately one to each ear of the subjects. Under such conditions the location of the source of sound seemed to fluctuate from right to left, always being on the side of the more advanced phase which of course shifted. Rayleigh also (cf. No. 75, for March) transmitted tones to the subjects' ears by means of two telephones, changing the phase by means of a commutator arrangement. The results agree with those of the first series of experiments. Lord Rayleigh concludes that "we are able to take account of phase-difference at the two ears."

In No. 76 (April, 1907) of the same magazine, L. T. Moore and H. S. Fry report experiments bearing on the same question. They transmitted one tone to the two ears by means of a Y-tube. When one of the branch tubes was made slightly longer than the other so that the wave reached the corresponding ear in later phase than in the other ear, the sound was located on the side of the short tube. These results, therefore, agree with those of Lord Rayleigh.

These results are, of course, inexplicable on the Helmholtzian theory, as on most others. Lord Rayleigh thinks that they uphold Rutherford's theory. This

And lastly, the experiment on the wave siren to ascertain whether differences of phase affect the nature of the tone, though it seems to point slightly to the affirmative, is not beyond question as to its validity. But the results obtained seem to indicate that phase differences are not so negative in their effect as Helmholtz supposed.

In 1881 Koenig again tried to show experimentally that beats when frequent enough can generate sensations of tone. He now employed his wave siren, already described. The simplest method employed was to draw out, on a large scale, two harmonic curves and then to construct from them the resultant compound curve by well-known methods of measurement. By constructing these curves on a large scale, Koenig was able to reduce errors. The drawing of the compound curve was then reduced by photography to the required dimensions. The curve thus constructed was then inverted and carefully reproduced on the edge of a metallic cylindrical hoop which was so mounted on an axis that it could be rotated rapidly. The reason for the inversion of the curve is that the elevations representing greater intensities would now give lesser intensities, on account of shutting off the current more than the depression would do. The instrument as used by Koenig had four such tooth-edged hoops, so that he could examine a number of intervals conveniently and compare results.[136]

When such a toothed rim is rotated before a slit fixed over it in the proper direction, and of a length at least equal to the greatest height of the curve, the slit will be periodically shortened and lengthened according to the law of the curve; and if wind is blown through the slit, a motion in the air must be generated corresponding to the same law. And this motion must be precisely the same as that produced by the simultaneous sounding of two really simple tones without any admixture of upper partials. The advantage of this arrangement, then,

theory supposes that the place of tone analysis is in the cortex and not in the ear, and really *explains* nothing. Certainly in these experiments the intensity difference and the possibility of bone conduction were not sufficiently controlled, and it is unsafe to place too much faith in the results until further experiments along the same line shall have been made.

[136] See Fig. 43, *Quelques Expériences d'Acoustique*, p. 160.

is that we know precisely what tones are acting and that they are undoubtedly simple.[137] So reasoned Koenig. He constructed disks and bands, as described above, giving the intervals 8:9, 8:10, 8:11, etc., up to 8:24. He thus describes his results: "The disks[138] for different intervals, when the rotation was slow gave beats, and when it was more rapid, beat-notes, exactly corresponding to those observed when two tuning-forks are sounded together. Thus the major second 8:9 produced the lower beat-note 1; the major seventh 8:15, the upper beat-note 1; the disturbed twelfth 8:23, the upper beat-note of the second period, which is again equal to 1, loudly and distinctly. In the same way the ratios 8:11 and 8:13 gave quite distinctly and at the same time the upper and lower beat-notes 3 and 5 for the first, and 5 and 3 for the second."[139]

"The evidence, then, that beats may coalesce and blend into a continuous tone is conclusive," says Zahm. "The more the movement of the air excited by impulses of any kind, differs from a simple pendular motion, the more these impulses will be separately distinguishable, and the less the sound due to their coalescence will be perceptible. On the other hand, the more nearly the periodic motion of the air approaches to a pendular motion, the less distinct will the separate impulses become, and the stronger the resultant tone. Wherefore, with the almost absolute pendular motion of tuning-forks, the separate impulses beyond 32 and 36 cease to be perceived, and the sound resulting therefrom predominates."[140]

This conclusion, which states essentially the view of Koenig, is no doubt *altogether too hasty*. While many good authorities agree with it in general, many others, on good grounds, consider that the experiment is by no means conclusive. "The question must still be regarded as an open one," is the opinion

[137] "But the possibility remains," says Lord Rayleigh, "that overtones, not audible except through their beats, may have arisen within the ear by transformation. This is the view favored by Bosanquet who has also made independent observations with results less difficult of accommodation to Helmholtz's view." *Op. cit.*, Vol. II., p. 469.

[138] He first used disks, later the hoops described above.

[139] Koenig, 'Ueber die Ursprung der Stösse und Stösstöne bei harmonischen Intervallen,' *Wied. Annal.*, XII., 1881, pp. 335-349.

[140] Zahm, *Sound and Music*, p. 338.

of Lord Rayleigh.[141] Concerning the views, held by the followers of Koenig, that the ear is capable of recognizing as a tone any periodicity within certain limits of frequency, Rayleigh suggests that a periodicity with frequency 128 is also periodicity with frequency 64. Is the latter tone to be heard as well as the former? Pipping, in 1895, urged the same point, that every periodicity of n is also a periodicity of $n/2$, $n/3$, $n/4$, etc.[142] "So far as theory is concerned, such questions are satisfactorily answered by Ohm's law," continues Lord Rayleigh. "Experiments may compel us to abandon this law, but it is well to remember that there is nothing to take its place."[143]

Koenig's wave siren experiments showed that at a certain rate of frequency both the beats and the tones, which he supposed to be generated by them, are heard at the same time. Koenig thinks that this is not necessarily contradictory to his view.

It must be kept in mind that Koenig still held to the *resonance hypothesis,* as did Young and others before Helmholtz, but did not accept Ohm's law. On this view, then, as explained by Zahm above, waves slightly diverging from pendularity are still sensed as tone. They are thus experienced, however, with much more difficulty than are pendular vibrations, while the beats are more easily perceived as distinct. In his later French publication[144] Koenig says on this matter: "At all events the simultaneous perception of separate beats and the sound which results from their succession is no more in contradiction with the new hypothesis than with the old [*i. e.,* Ohm's law as accepted by Helmholtz], for we can very well suppose that, besides the general excitement of the basilar membrane due to each separate beat, the particular parts of this membrane, whose proper tones correspond to the period

[141] Rayleigh, *ibid.,* p. 469.

[142] Pipping, Zur Lehre v. d. Vocalklängen, *Zeitschr f. Biologie,* XXXI.; N. F., XIII.

[143] Cf. Max Meyer's attempt at explaining another manner of analysis, page 92 f. of this paper.

[144] *Quelques Expériences d'Acoustique,* p. 137, trans. by Ellis in *Sensations of Tone,* p. 535.

of the impulses, are more strongly shaken, and excite lasting vibrations giving the perception of sound."

It will be recalled that Helmholtz had to account for all beats of wide intervals by the presence of upper partials or of difference tones. Now Koenig supposes that he has additional evidence against this view. From his approximately pure tones obtained from the resonated clangs, of large tuning-forks, he had heard beats that would require the presence of upper partials, which it would be entirely unreasonable to suppose were present.[145] Now, by means of the perfectly pure tones (as he supposed)[146] of the wave siren, he heard not only beats but also beat-tones, which according to Helmholtz's view would require the presence of upper partials. *E. g.*, 8:11 gave two tones 3 and 5 ($11 - 8 = 3$ and $2 \times 8 - 11 = 5$). 8:23 gave tone 1 ($3 \times 8 - 23 = 1$). But none of these tones are inexplicable from the formulæ of Helmholtz and Bosanquet.[147] And these formulæ resulting from the development of lower powers only do not exhaust the possibilities. In these cases, too, the resultant tones *do not depend upon upper partials, whether subjective or objective.* Even if no tone had before been heard corresponding to $3q - p$, *e. g.*, it is not an argument against the possibility that such tones exist, that Koenig now hears such a tone! Such tones (if indeed Koenig was not actually mistaken in the pitch, as others had been in regard to combination tones before him) are extremely rare whatever theory is adopted; and so far as the mere *existence* of these tones is concerned, they present no difficulty at all to Helmholtz's theory. Lastly we come to an important point in connection with this objection to Helmholtz's position. *How is Koenig to explain the perception of these tones?* It is well known that by means of his manometric flames and his complex curves, he finds periodicities corresponding to them; now, if he endeavors to make a mathematical or theoretical statement of these periodicities will he not actually obtain formulæ like those of Helmholtz and Bosanquet?

[145] Yet when Koenig comes to account for summation tones he posits the presence of partials of still much higher order! Cf. below pp. 53 f.

[146] But see note p. 48, *supra,* quoted from Rayleigh.

[147] Cf. p. 18 *supra.*

We have now to consider more especially the relation of the so-called beat-tones to combination tones. Koenig says that the beats (*i. e.*, the 'lower beats') are heard directly only to about the interval 4:7,[148] and, of course, beat-tones are heard only for smaller intervals. (The fact here is contradicted by recent experiments, as we shall see.) The interval $c^3:b^3$ (8:15), *e. g.*, gives only the tone 1, he says, and no trace of the tone 7 which Helmholtz's theory requires. So also with intervals beyond the octave; *e. g.*, the interval $c^3:d^4$ (4:9) gives only the tone 1 and never the tone 5; and $c^3:f^4$ (3:8) gives only 1 and 2. In short, the first difference tone of wide intervals, which Helmholtz's theory requires, is never heard by Koenig. None of Koenig's beat-tones fall between the primaries. Again there are no beat-tones corresponding to Helmholtz's *summation* tones. If then such difference tones and summation tones as Helmholtz believes in, exist at all, they are, for Koenig, distinct from the beat-tones. Only in certain cases do they coincide and in such cases their intensity is due largely to the presence of the beat-note of the same pitch.[149] "It follows," concludes Koenig, "that in all cases the differential tones must be infinitely more feeble than the tones resulting from the beats. But I was able to establish the actual existence of these differential tones with certainty by forming the above intervals with deeper notes, which, lasting longer, allowed me by means of auxiliary forks to get a definite number of beats with the differential tones in question."[150] In this paper, then, he does not deny the existence of combination tones, but holds only that they are very weak.

The summation tones, which Helmholtz heard easily from the siren and harmonium, Koenig explains as beat-tones of upper partials. These summation tones, while they may actually exist would be too weak to be heard. Now it is known, he points out, that the tones of the siren and of the harmonium

[148] *Quelques Expériences d'Acoustique,* note, p. 130.

[149] But cf. p. 105 below. Theoretically Helmholtz's theory includes all the tones heard by Koenig. It must be noted that Koenig did not always hear the tones required by his own theory, either.

[150] Koenig, 'Ueber den Zusammenklang Zweier Töne,' *Pogg. Annal.,* CLVII., 1876, p. 216. Quoted from Ellis' trans. in *Sensations of Tone.*

are not simple but are very rich in upper partials. The fifth, *e. g.*, with its two series of partials

$$\begin{array}{l} 2,\ 4,\ 6,\ \ 8,\ 10,\ \ldots \\ 3,\ 6,\ 9,\ 12,\ 15,\ \ldots, \end{array}$$

shows that the fifth partials will give a beat-tone ($15 - 10 = 5$) which is equal in pitch to the summation tone ($2 + 3 = 5$). The fourth ($3:4$) with the partials

$$\begin{array}{l} 3,\ 6,\ \ 9,\ 12,\ 15,\ 18,\ 21,\ \ldots \\ 4,\ 8,\ 12,\ 16,\ 20,\ 24,\ 28,\ \ldots, \end{array}$$

gives the tone 7 as a beat-tone of the seventh partials, and so on. In general the summation tones that are audible may be explained by the formula $n(a - b) = a + b$,[151] where a and b stand for the upper and lower primary tones respectively and n is some whole number.

In the French re-publication in 1882 the above observations still appear. Here, however, Koenig has enclosed in parenthesis the statement concerning his proof of the existence of combination tones by means of the beats of auxiliary forks, and on page 130 he has added the following long note. 'I have an important observation to make on all those remarks concerning differential and summation tones which are found in the first publication of the memoir in the *Annalen,* those which I have placed in parenthesis, but which I have reproduced here.

'New researches which I have made on beats of harmonical intervals since the publication of that memoir have demonstrated that even in forming very wide mistuned intervals, primary tones beat distinctly with a feeble auxiliary (excessive) tone; but, the auxiliary tones used to discover the existence of differential and summation tones, too feeble to be heard, were all harmonics of the tones of inferior beats, *i. e.*, of the lower beat-notes of the primary tones; and some of them, further, were harmonics of one of the primaries. They ought then, necessarily, to produce beats with these tones of which they were harmonics. Consequently from that time the beats

[151] Koenig does not seem to have used the formula $na - mb = a + b$; as a result he often has to posit the presence of very high upper partials to explain summation tones.

observed by me ceased to prove, in my judgment, the existence of corresponding tones interfered with (alteré) by the auxiliary forks, as I had previously held that they did.[152]

'A single exception should be made in case of the auxiliary of 440 v. s. which with the primary notes Ut_3 (8) and Si_3 (15) demonstrated by the presence of beats the existence of a feeble tone 7, which, however, was nothing but the tone of inferior beats (*i. e.*, the lower beat-note) of 8 and 15. Indeed the tables given above[153] show that the beats as well as the lower beat-notes of the first period can often be heard directly up to the neighborhood of 4:7, and it is conceivable that by the aid of an auxiliary tone, one should perceive them a degree further (14:15).

'After these considerations there remain only the two tones Mi_4 (5), produced by the interval $Ut_3:Sol_3$ (2:3), and $Ré_4$, from the interval $Ut_3:Mi_3$ (4:5), of which the existence may be regarded as really proved; because they have been observed directly by the ear. These tones, not having shown any action on resonators, as I have already indicated, cannot have the origin which Helmholtz attributes to differential and summation tones;[154] but, being very feeble, while the primary tones in the intervals which form them have a great intensity, they may be explained by the action of the feeble harmonics, produced in the ear by the primary tones; for according to Helmholtz, every very strong tone, even though simple, should produce harmonics in the organ of hearing, principally on account of the asymmetrical structure of the tympanic membrane, and partly on account of the loose articulation of the hammer with the anvil.[155]

'After what precedes, I know at present of no experiment by which one can prove with any certainty the existence of differential and of summation tones.'

In consequence of this 'discovery' he has revised note III.,

[152] It will be recalled that Koenig's experiments on beats lead him to the conclusion that one tone beats with its twelfth, *e. g.*, even though there are no upper partials present.

[153] *I. e.*, his tables on beats in *Quelques Expériences d'Acoustique*.

[154] This does not at all follow from Helmholtz's view.

[155] See my comment on this peculiar attitude of Koenig, page 56, below.

6,[156] of the 'conclusions' at the end of the chapter to read: 'The existence of differential and summation tones cannot at present be demonstrated with certainty by any experiment.' The original of this note (given in a footnote) reads: 'The differential and summation tones which are produced by the concurrence of two very strong tones, because the vibrations of the latter cease to be infinitely small, constitute a phenomenon which is independent of beats and of beat-tones.'

W. Preyer very early[157] raised the objection to Koenig's explanation of summation tones, that in some cases it requires the presence of a great number of overtones which apparently are not present. He cites the case of the interval 496:528 v. d. (31:33) of which he has heard the tone 1,024 (64). This would require the presence of the 32d partials, of 15,872 and 16,896 d. vib.[158] 'With tuning forks of 192 and 256 d. v. [3:4],' says Preyer, 'the summation tone 448 is heard clearly, even though both forks have been damped [to eliminate upper partials].' Now in this case it is very improbable that the seventh partial tones, after damping of the forks, would be loud enough to produce an audible beat-tone. In consequence of difficulties of this sort in the way of Koenig's view, Preyer refuses to accept it as a probable explanation. He is, however, of the opinion that these tones, though corresponding in pitch to summation tones, are too loud to be such. On the suggestion of G. Appunn, therefore, he shows that they may be explained as difference tones of the *second order; i. e.,* difference tones arising from the action of a difference tone on an upper partial. Thus instead of by means of the thirty-second partials of the tones of the intervals (31:33) referred to above, the summation tone can be accounted for as follows:

$$2 \times 528 - (528 - 496) = 1{,}056 - 32 = 1{,}024,$$

or

$$3 \times 528 - (2 \times 528 - 496) = 1{,}584 - 560 = 1{,}024.$$

[156] *Quelques Expériences d'Acoustique,* p. 147.

[157] *Akustische Untersuchungen,* 1879.

[158] Koenig thinks Preyer was deceived in the pitch in this case, that he heard really the octave of one of the primaries. Koenig, *Quelques Expériences d'Acoustique,* p. 127, note.

'Every term of these acoustical equations is easily proved [to exist].' Preyer's general formula then is expressed thus:

$$nb - [(n-1)b - a] = a + b.$$

Only the first overtone is necessary; *n*, then, $= 2$ and the formula becomes simply

$$2b - (b - a) = a + b.$$ [159]

When, however, b becomes greater than $2a$ the summation tone must be explained as a difference tone of the '*third*' order,

$$[nb - (n-1)a] - [(n-1)b - na] = a + b$$

or, when we consider only the first overtone,

$$(2b - a) - (b - 2a) = a + b.$$

'Since thus far neither I myself nor any one else has ever heard the summation tones when the first overtone was not at the same time very plainly audible, it is natural,' he says, 'to conceive of the summation tones as difference tones according to the above formulæ.'[160]

Now Koenig in turn, and on good grounds, objects to Preyer's explanation. To quote: "M. Preyer cites in favor of his views that on sounding together free reeds of 496 and 528 d. vib. $= 31:33$, he heard the sound 1024 d. vib. $= 64$, and he thinks that we cannot assume that the reeds had the thirty-second partials, 16,896 and 15,872 d. vib. If the sound really observed was 64, and not the octave of 31 or 33, we might be really astonished that the thirty-second partials were sufficiently strong in these tones to produce it; but the explanation proposed by M. Preyer is absolutely inadmissible, for 496 and 528 d. vib., even when they have considerable force, give 32 beats, which do not as yet allow the deep tone C_1 to be heard, so that at any rate such tone must be extremely weak. Now the octave of 528 (or 1,056) is the 33d harmonic of this excessively weak sound. But two primary sounds of 32 and 1,026 d. vib., even when extremely powerful, never pro-

[159] Röber (1856) first suggested this explanation, then independently G. Appunn, R. Fabri, and others. Cf. Stumpf, *Tonpsychologie*, II., 1890, p. 254.

[160] *Wied. Annal.*, XXXVIII., 1889, p. 135. I have not seen Preyer's earlier work.

duce a sound of 1,024 d. vib. The second manner in which M. Preyer thinks the sound might have been produced is equally opposed to all that has been directly observed when two primary tones sound together. Thus he makes the octave of 528 (*i. e.*, 1,056) produce with 496 d. vib. a differential tone of 560 d. vib., and then makes this tone 560 produce with the twelfth of 528 (*i. e.*, 1,584) a new differential tone of 1,024. But these two sounds of 496 and 1,056 ($= 2 \times 496 + 64$) give the beat note 64 and not 560; and if the sound 560 really existed, it would give with 1,584 ($= 2 \times 560 + 464 = 3 \times 560 - 96$) the beat-note 96, and also more faintly 464, but not 1,024."[161]

It is interesting to note that Koenig, when he fails to find objective upper partials for this explanation of summation tones, falls back for support[162] upon the subjective upper partials determined theoretically by Helmholtz and Bosanquet. The summation tones are, however, *co-equal* with the upper partials and not *dependent* upon them. Koenig, then, is in the peculiar position of accepting some of the values of x obtained from Helmholtz's equation and of rejecting others that stand co-equal with them! He would be far more consistent to go back to his earlier view and grant the existence of *weak* combination tones, if nothing more.

SECTION 2. THE OBJECTIVITY OF COMBINATION TONES.

Helmholtz, it will be remembered, had distinguished between combination tones that are *objective* and those that are *subjective*.[1] He had found that combination tones from primary tones of instruments having a common windchest were reinforced by resonators and that they were able to set in sympathetic vibration suitable membranes attuned to them. But even these he had found to be mostly subjective. Koenig in his first publication on the secondary phenomena of hearing,

[161] Translated by Ellis, *Sensations of Tone*, p. 536, from a footnote in *Quelques Expériences d'Acoustique*, pp. 127–8.

[162] Cf. *supra*, p. 53.

[1] Koenig, for one, had not kept this distinction clearly in mind.

when he still believed in the existence of combination tones, said that neither the combination tones nor the beat-notes described by him were reinforced by resonators.[2]

We have already quoted from Bosanquet, how by the use of his improved resonator, he shut out all tones but that to which the resonator was attuned, and that in so doing all beats and difference tones disappeared, thus proving them to be subjective.[3] Preyer, too, worked on this problem as to whether combination tones are objective.[4] He constructed for the purpose seven tuning forks of extraordinary delicacy: f 170⅔, c^1 256, f^1 341⅓, a^1 426⅔, c^2 512, f^2 682⅔, and g^2 768 d. vib. These forks form the ratios 2:3:4:5:6:8:9. They 'were so ready to vibrate on the slightest excitement that they could be experimented on at night only.' The three lowest forks had the following partials:

Fork f had the 1st upper partial f^1 strong, also the 2d c^2, and the 3d f^2 weak.

Fork c^1 had the 1st and 2d c^2 and g^2 strong.

Fork f^1 had the 1st f^2 strong.

The forks c^2 and f gave f^1 or $6 - 2 = 4$; f^2 and f gave c^2, or $8 - 2 = 6$; g^2 and c^1 gave c^2, or $9 - 3 = 6$; 'and that these tones were objective enough was shown by their making the forks f^1 and c^2 vibrate sympathetically. But we see that f^1 and c^2 are partials of f and c^1, which existed already strongly on those forks, and if the forks f and c^1 were sounded separately, they also made the forks f^1, c^2 vibrate sympathetically. Hence the results did not prove the objective existence of the differential duplicates. On the other hand the forks giving the audible differential tones—

$f^2 - c^2 = f$ or $8 - 6 = 2$ $\qquad$ $a^1 - c^1 = f$ or $5 - 3 = 2$

$g^2 - c^2 = c^1$ or $9 - 6 = 3$ $\qquad$ $a^1 - f = c^1$ or $5 - 2 = 3$

$g^2 - a^1 = f^1$ or $9 - 5 = 4$ $\qquad$ $g^2 - f^1 = a^1$ or $9 - 4 = 5$

$c^2 - f^1 = f$ or $6 - 4 = 2$

$f^2 - a^1 = c^1$ or $8 - 5 = 3$

$f^2 - c^1 = a^1$ or $8 - 3 = 5$

[2] *Pogg. Annal.*, CLVII., 1876, p. 221.

[3] *Supra*, p. 26.

[4] M. Preyer, *Akustische Untersuchungen*, 1879, II.

utterly failed to produce the slightest effect on the forks having the same pitch.'[5]

By a similar test Preyer satisfied himself that none of these forks gave objective summation tones. He did not, in fact, even find *subjective* summation tones, *i. e.*, he did not hear *any* summation tones. "Perhaps," says Preyer, "they might be made audible after properly arming the forks by means of resonance boxes *while* sounding. But the observation would not be easy."[6]

While Preyer's general conclusion is that combination tones are subjective, he acknowledges in a later article, from experiments which he there mentions, that appropriate conditions may be set up externally to the ear and made to generate combination tones.[7] Membranes, he says, may supply such conditions.

One of the experiments to which Preyer referred was that of O. Lummer,[8] in which, in Helmholtz's presence, Lummer made a microphone resonator respond perceptibly to the summation tone of primaries generated by the harmonium.

In the same year as Lummer's experiment, M. Wien obtained only negative results. He found that a very delicate resonator was never sensibly affected either by the loud difference tone of the primaries generated by two Quincke tubes (Lippenpfeifen) or by a tube and a telephone.[9]

L. Hermann, in 1891, reports an experiment bearing on the same problem of objectivity of combination tones. He admits that instruments with common windchest for both primary tones give objective difference tones, and therefore holds that Lummer's experiment is not to the point since he used the harmonium. The question, as clearly stated by Hermann, is, whether primaries of independent sources generate objective combination tones. 'I inserted,' says Hermann, 'two tele-

[5] Note that some of these 'audible differential tones' are intermediate difference tones.

[6] From Ellis, in *Sensations of Tone*, pp. 531–2.

[7] Preyer, Ueber Combinationstöne, *Wied. Annal.*, XXXVIII., 1889, p. 133.

[8] 'Ueber eine empfindl. obj. Klanganalyse,' *Verhandl. Berl. phys. Gesellsch.*, 1886, p. 66.

[9] Max Wien, 'Ueber Messung der Tonstärke,' *Wied. Annal.*, XXXII., 1889, p. 853.

phones into the same circuit with the electro-magnet of a Koenig secondary electric tuning fork, which was attuned to the tone *F*. The telephones *A* and *B* were in a separate room. Their steel membranes were removed and two assistants held the Koenig tuning forks a^1 and c^2 as near as possible to the coil. Never, however, was it possible in this way to bring into sympathetic vibration the fork *F* which corresponded to the difference tone of a^1 and c^2; but if another *F* fork was brought near one of the telephones, the electric fork vibrated weakly. On the other hand, if instead of the electric fork *F*, a telephone receiver (Hörtelephone) was inserted into the circuit, one could hear in it the difference tone *F* most beautifully; and likewise if, instead of a^1 and c^2, other forks were used, their difference tones or beats, as the case may be, were also heard. But this last experiment did not, indeed, prove that the plate of the telephone receiver produced or strengthened the difference tone; it simply reproduced to the ear simultaneously the two primary tones, and it was these which subjectively produced the difference tone.'[10] His experiment, then, seemed to show that generators of independent sources do *not* produce objective combination tones.

In 1895 the physicists A. W. Rücker and E. Edser[11] took up the problem and performed an experiment which has become classic. These men were stimulated to the investigation partly, at least, by the statements of Ellis in notes, pp. 156 and 157, of Helmholtz's *Sensations of Tone.* Ellis says there that it is probable that the 'apparent reinforcement' of the resonators noticed by Helmholtz, in case of combination tones from primaries generated by the siren or harmonium, 'arose from imperfect blocking of both ears when using them.'[12] "These statements are unqualified," they say, "and no condition was made as to the way in which the combination tones were pro-

[10] L. Hermann, 'Zur Theorie der Combinationstöne,' *Archiv f. d. ges Physiol.*, LIX., 1891, p. 516.

[11] Rücker and Edser, 'Objective Reality of Combination Tones,' *Phil. Mag.*, Series XXXIX., 1895, pp. 341–57.

[12] Ellis held the view of Koenig (for a time) and Bosanquet, and the one which Preyer also held for a time, that *all* combination tones (not 'beat tones,' of course) are subjective.

duced." They decided to make a careful test in which the ear need not be employed directly at all.

They used a tuning fork as resonator, a Koenig tuning fork of 64 d. vib. Since this instrument is relatively difficult to excite by resonance, they used a very delicate method of detecting whether it was set in motion. For this purpose a mirror was attached to one of the prongs and a system was formed by which the Michelson interference bands were produced. "A movement of the prong amounting to half a wave length of light (say 1/80,000 of an inch) would alter the length of the path of one of the interfering rays by a wave length. A periodic vibration of this amplitude would cause the band to disappear." The bands were sometimes produced by a sodium light, and sometimes by an electric light. They were watched by an observer through a telescope. A movement of "one hundred thousandth of an inch could easily be detected."

They first experimented with combination tones of primaries generated by the siren, i. e., where the primaries had a *common* wind supply. The experiment seems to have been conducted with the greatest of care. By accurate control of the revolutions made by the siren disk they could produce combination tones of pitch equal to that of the resonating fork. Many experiments were performed with different frequencies of the primaries, but in every case where the primaries formed intervals not greater than the octave, the fork was 'powerfully affected' when the difference tone had a frequency equal to its own.

The experimenters tested also for the first difference tone of an interval greater than the octave (4:9). According to Koenig's rules there are no 'beat-tones' of pitch intermediate between the primaries. The result of the experiment was unmistakable. When the primary tones reached frequencies such that the difference, 5, corresponded to 64 vibrations the resonating fork was disturbed as usual. "The effect was rather feebler" than in the other experiments with intervals smaller than the octave, "but there was absolutely no doubt as to the objective reality of the difference tone. The bands

regularly disappeared when the required pitch was obtained, and reappeared when it was lost."[13]

They tested next for Koenig's *lower beat-tone* with intervals greater than the octave. They produced for this purpose the tones 256 and 576 ($576 - 2 \times 256 = 64$ the pitch of the resonator fork). The results were always negative. "We lay less stress on negative than on positive results," they say; "but we tried for a long time on two occasions to get evidence of the objective character of the note, but entirely failed."[14]

They next turned their attention to the summation tone, still using the polyphonic siren with common windchest. Although the primary tones had to be so low that their sum = 64 v. d., the results were always unmistakable. When the pitch of the summation tone equaled that of the resonating fork the bands invariably disappeared. The experiments "left in the minds of those who saw them no shadow of doubt as to the objective reality of a note corresponding in frequency to the summation-tone."[15]

Koenig, it will be remembered, explained summation tones as 'beat-tones' of upper partials. Usually the 'beat-tones' of the lower harmonics are equal in pitch to the primary tones or to some of their upper partials. This is true, *e. g.*, of the fifth (2:3)

2, 4, 6, 8, 10
3, 6, 9, 12, 15.

Here $6 - 4 = 2$, the lower primary tone; $9 - 6 = 3$, the upper primary; $12 - 8 = 4$, the first upper partial of the lower primary tone; and $15 - 10 = 5$, the first such beat-tone of upper partials that we could expect to hear by itself.

The case, though, is somewhat different with the fourth (3:4)

3, 6, 9, 12, 15, 18, 21
4, 8, 12, 16, 20, 24, 28.

Here the 'beat-tones,' if such there be, of the first and fourth

[13] *Ibid.*, p. 350.
[14] *Ibid.*, p. 530.
[15] *Ibid.*, p. 351.

upper partials should be more prominent, presumably, than the 'beat-tone' of the sixth upper partial, which corresponds to the summation tone. Rücker and Edser found the tone 7, *i. e.*, the summation tone, to exist objectively, and decided now to test also for the tone 5 (from 20 — 15). This ought, according to Koenig's explanation, to be stronger than 7. "When the speed corresponding to this difference-tone [*i. e.*, 5] was attained, there were occasional flickers of the bands, so that it is possible that it has an objective existence. But on the other hand, the effect was less than that produced by the summation tone [*i. e.*, 7]. The bands never disappeared for any considerable length of time, as they did when the forks responded to the summation tone, and the experiment left no doubt in our minds that the greater effect was produced by the summation tone."[16]

Another slightly different test was made, bearing on the same problem, as to whether summation tones are due to upper partials. Not all primary tones have ratios that make it possible to explain the summation tone as produced by partials of the same order, *i. e.*, the equation

$$a + b = n(a - b)$$

does not always apply. If the primary tones have the ratio 9:16, *e. g.*, then $a + b = 25$ and $a - b = 7$. "The 10th partial of the upper note beating with the 15th of the lower note ($160 - 135 = 25$) would indeed have the same frequency as the summation tone, but it appears to be absurd to suppose that so improbable a combination should produce appreciable results. . . . If we assume that any pair of partials can thus produce objective tones, the number of combination tones will be so great that the fork ought to have been disturbed frequently when the note of the siren was being raised to the required pitch. *As a matter of fact when the C of 64 vibrations was passed, so that all the partials were higher than the pitch of the resonating forks, no such disturbances were ever observed except when the difference- or summation-tone of the primaries was produced.* Putting, therefore, all such fantastic

[16] *Ibid.*, p. 352.

combinations aside, the experiment may be regarded, as a test whether the summation tone can be produced when it cannot be due to two partials of the same order."

Testing, then, for the summation tone of the primaries of the ratio 9:16, they found, by the disappearance of the bands, that the summation tone, as in the previous cases, did actually exist objectively. To make sure that it was not the third partial of the lower tone, however, they made also a test for the partial. This partial was found to shake the bands a little when the rate of rotation made it equal to the pitch of the resonating fork, "*but the bands did not disappear, whereas they were completely wiped out by the summation tone when the two notes were sounded.*" Both by this means and by the difference in pitch of the two tones (one 25 and the other 27) the experimenters convinced themselves "that the effect of the two sources of disturbance could be distinguished, and that the supposed summation tone was not due to the partial of the lower note."[17]

Also by means of a *mirror-resonator,* constructed by Professor Boys to respond to a vibration frequency of 576, the experimenters demonstrated the objective existence of summation tones from primaries generated by a double siren with a common wind chest. These tests were made with primaries having the ratios 4:5, 3:4, and 9:16. In every case the summation tone disturbed the mirror.

"We attach great importance," say Rücker and Edser, "to this corroboration of our results by an instrument of totally different construction from that first employed."[18]

Experiments were also performed in which the primary tones were generated by tuning forks, but the results were negative. "No effect whatever was produced [on the resonating fork], and there can be no doubt that if objective combination tones are produced in such cases they are much less intense than those generated by the siren." Tests were also made with reed tones and with tones of organ pipes. In the first case the results were uncertain; in the second they were negative.

[17] *Ibid.*, pp. 352–4 (italics mine).
[18] *Ibid.*, p. 354.

Several attempts were made to detect combination tones of higher orders, such as $2q + p$ and $2q - p$, but without success.[19] No statement is made as to how the primary tones were generated in these cases. They were presumably tones from the siren.

Rücker and Edser conclude, on good grounds, that Helmholtz was right in ascribing objective reality, in part, to the first combination tone ($p \pm q$) of primary tones produced by the siren. So far as the negative results are concerned, it can only be said that the *instrument used* was unable to show any objective reality of the tones sought.

Forsyth and Sowter,[20] by means of photography, registered the sympathetic vibrations of a small mirror to combination tones generated by the siren. The instrument was so delicate that they found it necessary to make their tests at night when traffic had subsided. The mirror was attuned to 64 vibrations per second. The primary tones were produced by a Helmholtz polyphonic siren. When a 64-fork was sounded, the experimenters obtained (by instantaneous photography) a series of sinus lines, which were used for comparison with results obtained both from difference tones and from summation tones when the frequencies equaled that of the natural period of the mirror. The photographs show that the summation tone was somewhat weaker than the difference tone. Only a few intervals were used, but there remained no doubt as to the results obtained.

In 1899 K. L. Schaefer records[21] results of experiments which he performed on the harmonium and the Appunn triad apparatus (Dreiklangapparat). He was able to reinforce with resonators not only the first combination tones ($p \pm q$) but also the so-called difference tone of the second order ($2q - p$). In relation to the intensity of the tone, the rein-

[19] *Ibid.*, pp. 355–6.

[20] R. W. Forsyth and R. J. Sowter, On Photographic Evidence of the Objective Reality of Combination Tones. *Proceedings of the Royal Society of London*, LXIII., 1898, pp. 396–399.

[21] K. L. Schaefer, 'Eine neue Erklärung der sub. Combinationstöne auf Grund d. Helmholtz'chen Resonanzhypothesis,' Pfluger's *Archiv*, LXXVIII., 1899, 508–526.

forcement was not great but unmistakable. Schaefer found, too, that no reinforcement was possible when the primaries were generated from separate wind chests. He further agreed with Helmholtz's results in finding that the degree of strengthening of the combination tones by resonators is always small in comparison with the intensity with which the tones are normally heard without resonators. The greater part of their intensity, he concludes, is, therefore, not of objective origin.[22]

From the above experiments bearing on the question of the objectivity of combination tones it may safely be inferred that if combination tones from primaries generated independently exist objectively at all, they are extremely weak. Lord Rayleigh assures us, however, that owing to the want of symmetry due to condensation and rarefaction in the air "the formation to some degree of octaves and combination tones is a mathematical necessity."[23]

It is at once evident that the question of the origin of the secondary phenomena of hearing is closely connected with—is indeed only another phase of—that which we have just been discussing. If the combination tones, *e. g.*, could all be shown to exist objectively the question as to their cause might well be left entirely for physicists to answer. It is an appreciation of this fact that has caused several acousticians in recent years to investigate rather carefully the question of objectivity with reference to various so-called resultant tones. We shall return now to the subject of 'interruption tones' and of variation tones and see what effect the question of objectivity has had on the development of their theoretical treatment.

[22] K. L. Schaefer, 'Weiter Bemerkung zu meiner neuen Erklärung, etc.,' Pflüger's *Archiv*, LXXXIII., 1900, 73–78, cf. pp. 75–6.

[23] *Theory of Sound*, II., 1896, p. 459.

SECTION 3. Later Experiments on Interruption Tones.

We saw that the earlier studies of the periodically variable tones were made by physicists, and that they had come to a rather general conclusion that if a tone n is interrupted periodically m times in a certain period of time, other tones, corresponding to $n + m$ and $n - m$ should arise as actual pendular vibrations in the air; and that in some cases these tones had actually been located as to their pitch by the aid of resonators, though the question of objectivity had not been explicitly considered. Several investigators had also noticed a tone corresponding to m, though the results of the mathematical determinations did not show such a vibration. The tone m, moreover, had apparently never been resonated objectively.

In 1887 H. Dennert[1] took up the problem where Koenig had left it. Dennert's own description of method and results follows: 'On a disk with three circles which were each divided into 96 equal parts,' he so constructed holes of equal diameter 'that on the first circle 4 parts that were perforated regularly alternated with 4 unperforated ones; on the second circle 3 perforated parts, with 3 unperforated ones; on the third 2 perforated, with 2 unperforated parts. Therefore on the first circle were twelve groups of 4 holes with 4 blank spaces between each group, in the second 16 groups of 3 holes with 3 blank spaces between each group, and on the third 24 groups of 2 holes with 2 blank spaces between each group. Now if the circles were blown upon [through a tube] during the rotation of the disk, one heard, with slow rotation, beats which, with more rapid rotation, merged into tones. The beats of the third circle first went into a tone, then those of the second, and lastly those of the first. In every phase of the investigation the tones stood in the relation 1 : 4/3 : 2, so that when the lowest of the tones from the beats was equal to c, one heard, besides the tone c^3, which corresponded to the 96 holes, also the three tones in the series c, f, c^1.'[2]

[1] H. Dennert, 'Akustische-physiol. Untersuchungen,' *Archiv für Ohrenheilkunde*, XXIV., 1887, pp. 171 ff.

[2] *Ibid.*, p. 181.

For Dennert these last three tones, c, f, c^1, had no corresponding pendular vibrations in the air, but, as for Koenig, were supposed to be generated from rapid beats due to the periodic interruptions of the tone c^3.

Hermann, also a supporter of the 'beat-tone' view, next took up some rather extended experiments on the 'interruption' tone and on allied problems. First a constant tone f^2 was conducted through a tube to the observer's ear and a revolving disk with a circle of 18 holes of 16.5 mm. diameter was arranged between two sections of the tube so that it periodically interrupted the tone. When the rotation was rapid enough the beats of intermittence merged into a continuous low tone, which, on account of the noise of the rotating disk, was not very clear. When the rotation was sufficiently increased the high tone f^2 became entirely inaudible.

In a second experiment a tone transmitted by telephone was interrupted by means of a water motor. Again when the interruption was sufficiently rapid a low dull tone was heard corresponding in pitch to the number of interruptions. With greater frequency of interruption this low tone also completely drowned out the high tone. In both of these cases, however, the low tones disappeared when the high tone was stopped,[3] showing their dependence upon it.

In another experiment Hermann employed four of the Savart toothed wheels (Zahnräder) having 80, 60, 50 and 40 teeth respectively. The wheel with eighty teeth had ten blank spaces which divided the teeth into eight groups with equal distances between each two successive teeth. With each revolution, therefore, there would be ten interruptions of the tone produced, when the edge of a piece of paper was brought into contact with the teeth. When the wheel was rotated ten times per second, *e. g.*, there was heard, besides the higher tone of 800 vibrations, also a low tone corresponding to 100. In this case also the high tone disappeared on very rapid rotation, while the lower, or 'interruption tone,' only was heard. Now when the disk with 60 teeth was likewise arranged for ten interruptions per revolution, it was found, on making an equal num-

[3] Hermann, *op. cit.*, pp. 385–86.

ber of revolutions with the first, to produce a low tone of the same pitch as that of the first disk. The two higher tones were, of course, different, bearing the ratio 3:4. By the use of the remaining disks several experiments were performed with similar results. After a rapid rotation one notices, as the disk gradually loses speed, that the upper tone comes out more clearly as the lower one falls in pitch. Finally the lower one ceases and only beats of the interruptions are heard. Very rapid rotation always drowns out the high tone, as in the previous experiments. It is clear from this change of the lower tone into beats, argues Hermann, that the ear perceives rapid beats as a continuous tone.[4]

W. Voigt,[5] at about the same time that these experiments were in progress, endeavored to construct a theory on the basis of mathematical determinations, which should take into account two points in which he thought the theory of Helmholtz was insufficient. (1) Koenig and others had shown to Voigt's satisfaction that every periodic fluctuation in the air was sensed by the ear as a tone.[6] This, of course, the Helmholtzian theory denied. (2) *Difference tones,* as Voigt himself found, ***do not always have the intensity that Helmholtz's theory demands.*** On the assumption, then, that any periodicity may be sensed as a tone, and rejecting the disturbance of vibration by superposition, Voigt obtains results that admit both of Helmholtz's combination tones and of Koenig's 'beat-tones.' Really, however, as Krueger suggests,[7] Voigt gives up the Ohm-Helmholtz theory of analysis without explicitly recognizing it. Voigt finds from theoretical considerations that the summation tone should not exist for intervals of the octave, the fifth, the fourth and the third, and that its existence in the case of the major sixth, and the twelfth is very questionable.

[4] *Ibid.*, pp. 386–7.

[5] 'Ueber die Zusammenklang zweier einfacher Töne,' *Wied. Annal.*, XL., 1890, pp. 652–60.

[6] This statement must be understood as applying to the rates of vibration within audible limits, *e. g.*, 16 to 50,000 per second.

[7] *Phil. Studien*, XVII., 1901, p. 265.

SECTION 4. Further Criticisms and Modifications of Helmholtz's View.

Hermann now[1] decides that Helmholtz's theory, 'beautiful as it is' is inadequate to explain the empirical facts that have been gathered. Koenig, Dennert, and himself have found that the beats of periodic interruption produce sensations of continuous tone; Koenig found that the difference tone corresponding to $(p - q)$ is heard only for intervals not much wider than half (?) an octave. Voigt has pointed out that the intensity relations of combination tones do not agree with the demands of the Helmholtzian theory; and Hermann himself "had never heard summation tones and had never found any one who could hear them" even under the most favorable circumstances.[2] In addition to Voigt's observation on intensity Hermann has himself observed that the forks $c^2:e^3$ on resonance boxes give the difference tone F on very gentle sounding. '*This simple fact in itself,*' he says, '*is sufficient to disprove the Helmholtzian theory of combination tones.*'[3]

But Hermann has still other objections to urge. Difference tones, he found, can easily be heard even when the ears are stuffed with cotton or filled with a wax compound,[4] so that the drum can not function as Helmholtz's theory demands that it should for the generation of combination tones, by transformation of the primaries. Furthermore, Hermann found that on having the tones of two tuning forks conducted one to each ear through pipes, he still heard both beats and combination tones.[5] In this case he supposed that the tones, through the mediation of the bones of the head, both act together in each ear, but that difference-tone origin in the drum is impossible.

Aside from these difficulties, Hermann also suggests that Helmholtz's resonance theory is improbable from the fact that it requires fibers hardly 0.5 mm. long to respond sympathetically

[1] 'Zur Theorie der Combinationstöne,' *Pflügers Archiv,* XLIX., 1891, pp. 499–518.

[2] *Ibid.,* p. 500.

[3] *Ibid.,* p. 512.

[4] *Ibid.,* pp. 512–3.

[5] See the interesting experiment by Cross and Goodwin, p. 75 ff., below.

to tones of less than forty vibrations per second. Such a thing he believes to be contradictory to what we know of sympathetic vibration.[6]

Now, in the face of this evidence against the view of Helmholtz, Hermann decides to abandon it. 'Nothing remains then,' he says, 'but to return to the old natural deduction of difference tones from beats, *i. e., to ascribe to the ear the power of responding with the sensation of tone to every kind of periodicity within certain frequency limits.*'[7] In this new creed he hoped to find peace of mind with the difficulties enumerated above.

But while we are compelled to put away the resonance hypothesis, the principle of the specific energies of nerves remains unaffected, he continues. No experiments have yet made the application of this principle to tone analysis improbable. We are forced, then, in the case of hearing, as we are in other senses, to abandon the hope of explaining *how* a certain tone exclusively or preferably stimulates a certain nerve.

Now with this view that the ear perceives as tone not only pendular vibrations, but also *any* periodic vibration, within certain frequency limits, Hermann took up a study of other possible tones.[8]

If we construct the resultant of two curves of equal amplitudes representing frequencies of m and n we obtain a curve that approaches sinuosity, with the exception that its amplitude fluctuates periodically from zero to the sum of the amplitudes of the curves m and n. The period of this resultant curve corresponds to the arithmetical mean $(m+n)/2$ of the primaries and is, therefore, by Hermann, called the 'Mittelton.'[9] Koenig referred to it as the 'son moyen.'[10] If now m and n are the vibration numbers of the primary tones respectively in 2π seconds, and if these tones are conceived as cosinus curves their formula, beginning with the opposite phase, is

$$a \cos mt - a \cos nt,$$

[6] Helmholtz anticipated this objection himself; see above, p. 13.

[7] *Ibid.*, pp. 513–4.

[8] Hermann, 'Beiträge zur der Lehre von der Klangwahrnehmung,' *Pflüger's Archiv*, LVI., 1894, pp. 467–99.

[9] *Ibid.*, p. 486.

[10] *Quelques Expériences d'Acoustique*, p. 143.

where m is greater than n; or by simple trigonometric conversion,

$$a(\cos mt - \cos nt) = - 2a \sin \frac{m+n}{2} t \sin \frac{m-n}{2} t$$

In this case $(m + n)/2$ is the vibration number of the 'middle tone' and $\sin (m - n)/2$ represents the intensity of the fluctuation of the tone, and evidently will change sign periodically. As Lord Rayleigh assures us,[11] *this change of sign* is equivalent to a *change of phase*. The 'middle tone,' then will periodically change its phase, *i. e.*, in every 2 seconds. It is evident that between each two phase reversals of this 'middle tone' only a relatively small number of vibrations can take place. The general expression of this number is obtained by dividing $\frac{1}{2}(m + n)$ by $(m - n)$ and is, therefore,

$$\frac{m+n}{2(m-n)}$$

as Hermann shows.

It is evident that this 'middle tone' with changing phase will not act on a resonator of high degree of resonance where the cumulative effect of a great number of impulses is important. In such a case, *e. g.*, as the resonance of a tuning fork, the second series of impulses with a reversal of phase would tend to neutralize the effect of the first. The effect, if any there be, of the 'middle tone,' for this reason, would be greatest in cases where the resonators would soon be damped, *i. e.*, where resonance is of a low degree. It will be easily understood now why the 'middle tone' is not susceptible of reinforcement by physical resonators. Still Hermann thought that he had occasionally heard this tone in cases of interference of primaries forming simple intervals.

By different arrangements of teeth in the toothed siren Hermann found (1) that periodic vibration may be perceived as a tone even though there is a regularly recurring change of phase which does not occur with too great frequency. For example, a periodicity changing its phase with every 16th vibration is sensed as a tone. The limits of tonal perception in

[11] *Supra*, p. 38; cf. also Hermann's figure, *Pflüger's Archiv*, LVI., p. 485.

such cases were found to be at about four vibrations between each change of phase. He found (2) that in cases of periodic change of phase, as well as in periodic intermittence, a tone may arise whose vibration number equals the number of phase reversals. Accordingly *he explained difference tones as resulting from this periodic fluctuation of phase and amplitude of the 'middle tone.'* The period of the fluctuation is expressed by the difference $m - n$, and, moreover, the number of the vibrations of the 'middle tone' within each phase becomes so small for wide intervals that it renders the existence of the difference tone in such cases impossible. He was thus able, on the basis of his precarious assumptions, to explain another phenomenon which he held as contradictory to the Helmholtzian theory.[12]

Now how shall we explain the fact that a periodicity may be sensed as a tone even though the change of phase occurs as often as once in every four vibrations [see (1) above]? If stimulation of the nerve endings is effected entirely by forced vibrations we should suppose that a change would not affect the tonal perception at all. On the other hand, if stimulation is effected by the presence in the cochlea of perfect resonators the effect of phase change ought to be *much greater than it is.* Hermann, though he had rejected Helmholtz's theory of difference tones altogether, decides to posit the presence of certain imperfect resonators in the ear. Since change of phase is opposed to resonance it is natural that the 'middle tone' should only seldom succeed in getting any sympathetic response in the cochlear resonators. Thus he accounts for the fact that this tone is seldom heard.

Now, however, he is confronted with the same difficulty that caused Helmholtz to reject the view that rapid beats merge into continuous tone. This view is opposed to specific energy of nerves and resonance, in that each nerve is affected only by its own resonator. To get around this difficulty Hermann assumes that each resonator operates not directly on the nerve, but upon the nerve cell and thus indirectly upon the nerve; and *that each of these cells is functionally connected with every*

[12] But how will *he* explain the fact that the intensity of the second difference tone sometimes exceeds that of the first?

resonator. It matters not, then, *where* in the series the vibration sympathetic to the given tone is, for every resonator communicates with the only nerve that can mediate the corresponding tone.

In 1896 Max Meyer reports[13] experiments on the toothed and also on the perforated siren that confirm the results of the similar experiments made by Hermann. But Meyer points out the fact that these experiments do not necessarily support the 'middle tone' theory of Hermann. The phase-changing vibrations produced on the siren are all of approximately equal amplitudes, whereas those of the 'middle tone' vary between 0 and a certain maximum value. Meyer found that on faster rotation of the siren the phase-changing tone gradually gave place to the variation tones which finally survived alone, while the former disappeared altogether. Meyer suggests that what Hermann heard and interpreted as the 'middle tone' may have been upper partials of the primaries which in some cases correspond to harmonics of the supposed 'middle tone.' *E. g.*, the 'middle tone' of $c^1 : g^1$ is e^1, which might have been suggested by the upper partial e^3.[14]

It is important to note that Hermann did not seem absolutely certain himself that he heard the tone in question. Any one who knows the force of imagination in trying to hear a weak tone of a clang, especially when the pitch of the expected partial is known, will feel inclined to doubt under the circumstances that Hermann actually heard the 'middle tone.' Krueger[15] says that the existence of such a tone becomes more improbable as experimental conditions approach the actual conditions demanded by Hermann's theory.

Wundt in 1893[16] suggests, as a supplement to Helmholtz's theory, an hypothesis quite different from the theory of Hermann. With Koenig, Hermann, and other, he agrees that Helmholtz's view cannot explain certain secondary phenomena of hearing, such as have been pointed out by Hermann. Cross

[13] M. Meyer, 'Ueber Combinationstöne,' *Zeitschrift f. Psychol.*, IX., 1896, 177–229.

[14] *Ibid.*, 196 ff.

[15] *Phil. Studien*, XVII., 1901, p. 270.

[16] W. Wundt, *Phil. Studien*, VIII., pp. 641–52.

and Goodwin[17] heard beats from soft tones not far separated in pitch, even when one tone was separately conducted to each ear, precautions being taken against the conduction through the bones of the head. Such results, holds Wundt, are impossible if each nerve fiber, however stimulated, can mediate only its particular sensation. Beats of wide intervals, such as Koenig heard, are inexplicable on this theory. These difficulties can be overcome, says Wundt, by the simple assumption that the *vibrations of both tones can somehow directly stimulate all the fibers of the auditory nerve,* without going to or acting through the resonance apparatus in the ear. In case of direct stimulation of this kind, such intermissions of stimuli as are objectively not in a condition to produce a tone could set up together the conditions necessary for a tone sensation.

This supposition is opposed to that of Hermann in that (1) it retains the resonance hypothesis as formulated by Helmholtz and also his explanation of beats and combination tones. 'For any mechanically comprehensible interpretation of the resonance hypothesis, this direct stimulation of the nerve of hearing is not in opposition to the resonance hypothesis, but must stand to complete it; the hypothesis demands it.' (2) It does *not* hold to specific energies of nerves in any strict sense. On both of these points, it will be remembered, Hermann took the position just opposite to that of Wundt. If structures attuned to repond to certain objective vibrations can stimulate the nerve fiber by sympathetic vibration, argues Wundt, why cannot the objective vibration directly stimulate the nerve? R. Ewald showed that animals from which the whole hearing labyrinth has been extirpated, still react to sound stimuli even in cases where the tactile stimuli were supposedly excluded.[18] Even though he attributes the response to *general* nerve stimulation, that does not exclude the kind of direct stimulation here assumed. While other nerves, *e. g.,* those of touch, are not excitable by so low intensity of stimulation, we may well sup-

[17] *Proc. of the American Acad. of Arts and Science,* XXVII, June 10, 1891.

[18] R. Ewald, *Physiol. Untersuchungen über das Endorgan des Nervus Octavus,* Wiesbaden, 1892, p. 29. It has usually been supposed by critics, however, that Ewald's animals reacted to tactile stimulation.

pose that the auditory nerve through adaptation is more sensitive to such stimulation.

On the anatomical side Wundt finds further confirmation of his view. The auditory nerve shows a divergence from all the nerves in that just before its peripheral termination it does not, like other nerves, become imbedded in soft tissue, but unravels itself into fine fibers surrounded with bony walls. This complexity of structure and this type of protection are unmeaning from the view that tonal analysis takes place solely in the arches of Corti. But, says Wundt, useless structures are not found in organisms (!). It is well known, moreover, that tones may be mediated through the bones of the head.

If, then, this 'supplementary hypothesis' is correct, every sensation set up by the direct stimulation of the auditory nerve will conform to the objective form of the wave. Simple tones will be sensed as simple; complex, as complex. In cases of interference, beats and beat tones will arise. Thus, not rejecting Helmholtz's theory, we may admit with Koenig that beats when frequent enough may be sensed as tones.

Of this 'supplementary hypothesis' of Wundt we need say only a few words. It is a strange mixture of the doctrine of specific energy of nervous function in the cochlea, with an entirely different view of the *modus operandi* of the *same nerves* where they pass through the bony structure. The contradiction of the 'supplement' with the specific energy theory of Helmholtz is so great as entirely to do away with the view which it is to complete! The two suppositions cannot keep house together on any known principles of the physiology of nerves. Moreover, Wundt himself concedes, later, on the basis of experiments which we shall soon consider, that the so-called beat-tones and intermittence-tones 'have themselves been reduced with great probability to difference tones.'[19]

It is important to note, in connection with the criticisms just urged against Wundt's "supplement," that the results of the experiment of Cross and Goodwin,[20] *taken in their entirety,*

[19] Wundt, *Physiol.-Psychol.,* 5th ed., Vol. II., p. 137.

[20] Chas. R. Cross and H. M. Goodwin, 'Some Considerations Regarding Helmholtz's Theory of Consonance,' *Proc. of the Am. Acad. of Arts and Sciences,* XXVII., 1891, pp. 1–12.

contradict Wundt's position, although Wundt takes consolation from a certain *part* of the results. We shall, therefore, examine a little more carefully the results and the conditions of this experiment.

An " effectual means of making audible very small vibrations is to close the ear with a bit of beeswax and press the stem of the fork lightly against the wax. In this case the vibrations are transmitted to the membrana tympani by the small amount of air enclosed within the meatus, as is clear from the fact that the sound of the fork is heard on touching the wax long after it ceases to be audible on touching its stem to the pinna of the ear. Hence in this case there is no conduction to the middle ear or inner ear through the bones of the head [this cannot yet be asserted beyond question]. Now we found that the vibrations of a fork could be heard longer when touched to the wax in the ear than when held against the teeth. We therefore took two small tonometer forks making four beats per second, struck them very gently, and held their stems against the teeth; loud beats were heard in the ears. . . . The forks were then held in this position until the beats had entirely ceased to be audible, when they were removed, and the stem of each was touched to the wax closing the two ears. Instantly the two notes were heard, faintly but distinctly, in the ears to which they were held, and accompanying them were faint beats seeming to wander in the head from ear to ear, as is always the case with binaural beats." The beats were correctly counted by the subject.

" The experiment was varied slightly as follows: One ear only was closed with wax; the other was immersed in a large basin of water. The experiment was then repeated as above, with the difference that one fork, instead of being touched to the ear was touched to the marble basin, its vibrations being transmitted to the enclosed ear through the water. The same results were obtained as before." The experimenters " conclude that aerial vibrations acting upon the ear are not transmitted through the skull or bony parts of the head from one ear to the other."

Upon this part of the results of the experiment, then, Wundt

largely bases his theory. But the experimenters go on, and their results agree in general, as they point out, with those of similar experiments by Thompson in 1881. "The ears being closed with wax, a brass rod about five feet long was held lightly against the wax in each. When the stems of forks struck by two assistants were pressed against the farther end of the rods, very loud tones were heard in the ears, unaccompanied by any differential tone. If, however, one of the rods was removed from the ear and pressed tightly against the head, or, better, against the teeth, a loud differential tone was heard at once in the ear against which the rod was placed. If both rods were held against the teeth or head, the differential tone was heard in both ears." Difference tones were, therefore, readily produced whenever bone conduction was made possible *so that both tones might operate in the same inner ear.* These results were confirmed by several other different tests which need not be described.

The experimenters conclude that while Helmholtz's explanation of beats may be partly right, the production of beats is in part due to the condition resulting in the sensorium itself when two interfering tones are sensed. They offer no explanation of the origin of combination tones, though they find, contrary to their expectations—based upon Helmholtz's explanation of asymmetry in the tympanum—that such tones arise even when it is impossible for both generating tones to operate together on either tympanum.

These results, bearing on the hearing of difference tones, certainly contradict the very fundamentals of Wundt's "supplement" to Helmholtz's theory. On the other hand, they are in agreement with the modification of this theory suggested below, page 104.

As early as 1890 Stumpf pointed out a phenomenon which had hitherto not been mentioned by acousticians, and which seems in a measure to conflict with Ohm's law as interpreted by Helmholtz. "(*a*) If I take two tones about a semi-tone apart in the middle region of the scale (*e. g.*, g^1 and a^1 on the violin)," says Stumpf, "I hear the two primary tones, but also, over and above these, a third tone which lies between them,

somewhat nearer the lower than the higher. This third tone has a very soft coloring, and with keen attention is localized within the ear; it is this tone which beats, while the primary tones remain constant. The two primary tones are, in my judgment, noticeably weakened,—more than is customary when two tones are sounded at the same time.

"(*b*) If I take tones that lie farther apart, in the same region of the scale (*e. g.*, g^1 and a^1), I do not hear any middle tone, but only two primaries; and these two seem themselves to beat. If, however, I turn the attention more particularly to one of them, this always seems to be the beating tone.

"(*c*) If, on the other hand, I take two tones that lie much nearer together than a musical semi-tone, so that they approximate the difference limen for simultaneous tones, I get one tone, and that beating. It is difficult to say whether it lies between the primaries."[21]

As is well known, Stumpf attempts to explain this phenomenon of the so-called intertone (Zwischenton)[22] on the basis of an assumed 'physiological accommodation.' On this principle all the fibers affected by a certain given tone mediate the sensation corresponding to that tone, and *not* each the tone of its own period. If this supposition be not made, we should expect to hear a number of tones of different intensities for each single pendular vibration. Now, he says, the intertone of case (*a*), described above, is produced in this way: The two vibrating sections of the basilar membrane, corresponding to the given tones, overlap. In this condition there will be one intermediate nerve fiber equally affected, and more intensively than others, by both forms of stimulation. This fiber will then mediate the sensation of the intertone. This fiber may be supposed to constrain the neighboring fibers or cells in the direction of its own specific energy. The nervous structures excited will then fall into three groups—the upper and the lower correspond to the primary tones; the intermediate, to the *intertone*. The outside tones will be weakened by the loss of the inner vibra-

[21] C. Stumpf, *Tonpsychologie,* II., 1890, 480–1, translated by Bentley and Titchener, *Am. Jour. of Psychol.,* XV., 1904, 66–7.

[22] This cannot be translated '*middle tone.*' That would confound it with Hermann's 'mittelton' which I have thus translated.

tions contributing to the intertone, and the interference of the two modes of stimulation will affect this tone and make it beat. In the case of (*b*) the overlapping is too slight for the production of an intertone. Where the primaries are very near together in pitch the three tones simply fuse together.

Ebbinghaus accepts Stumpf's description of the phenomena, and wrongly supposes[23] that an intertone, such as is described, is to be expected on the Helmholtzian view. "The *Zwischenton* is precisely what the Helmholtz theory does *not* explain; it is precisely what we should *not* expect from that theory."[24] But it is to be noted that Ebbinghaus himself does not attempt to explain how such a tone is produced. He *has* no explanation. His theory admits of none. We shall briefly consider his theory.

Helmholtz's theory cannot explain the beats of wide intervals, he says: it cannot explain the origin of 'interruption tones,' which tones he still considers, with Koenig and Hermann, to be subjective,[25] and to arise from a rapid 'beating' of the given tone. He objects, moreover, to an explanation of combination tones which makes these tones so different in origin from beats as Helmholtz conceived the matter. Hermann's modifications are unnecessarily complex,[26] so Ebbinghaus proposes the following.

The general theory of analysis as defined by Helmholtz is good. It is inconceivable, however, that the nervous elements are from the first as closely specialized as Helmholtz's theory supposes. We may conceive of the cells of the cochlear nerve as at first non-specialized. *Each* cell may be able to mediate *every* tone sensation. The close association of each cell with a resonator of a particular period, however, causes it gradually

[23] Ebbinghaus, *Grundzüge der Psychologie*, 1902, p. 317. All the page references to Ebbinghaus' *Psychologie* refer to the first edition. The second edition (1905), which I did not have when the above was written, contains nothing necessitating any changes in Ebbinghaus' theory as stated here. In the later edition Ebbinghaus has, however, left out his objection that Helmholtz's theory cannot explain the origin of 'interruption tones,' but he objects to Helmholtz's explanation of beats and some related phenomena, p. 334.

[24] Bentley and Titchener, *op. cit.*, p. 69.

[25] Ebbinghaus, *op. cit.*, pp. 312–3. But see note 23, above.

[26] *Ibid.*, note, p. 317.

to acquire a special physiological habit for a certain condition of stimulation. It comes to respond most strongly to the period of its own resonator, though it still *can* respond to other periods. These other periods of stimulation may come to it in two ways: (1) by the weak response of its own resonator to tones of nearly the same frequency as its own, (2) by partial vibration. 'Every single tone-wave striking the basilar membrane *sets into sympathetic vibration not only the fibers directly attuned to it, but also, to a certain extent, all those fibers which are attuned to* its harmonic undertones—these, of course, into partial vibration by the formation of nodal points.' If, *e. g.*, a tone of 600 vibrations is given, it will set into partial vibration the fibers corresponding to the periods 300, 200, 150, 120, etc. *Now, for Ebbinghaus, pitch is determined directly by the frequency of the stimulation;* hence *all* of these cells when stimulated together by partial vibrations will mediate the same tone (*e. g.*, the one corresponding to 600 in the above case). Each cell then comes to respond relatively easily to the octave of its own period, less easily to the twelfth, and so on.

Now, *beating* is explained on the principle of interference of vibrations in the basilar membrane fibers, as it was by Helmholtz. Each of two interfering tones is thus periodically strengthened and weakened. In case of wide intervals interference is possible through partial vibrations of fibers corresponding to harmonic undertones of the given tone. Thus the difficulty of Helmholtz's theory of beats is overcome. But now just how these beats are mediated (*i. e., what* fibers mediate them and *how*), is the question. The *bearers* of the beats, says Ebbinghaus,[27] are not the intermediate overlapping fibers, but those directly correlated with the stimulus rhythms. The tones mediated by these fibers are then made to fluctuate periodically in intensity. When fast enough these beats, as Koenig held, may be sensed as difference tones. Here then we seem to have the anomaly of two tones mediated by the same fiber or cell. Perhaps not, for the higher tone may be of a period too frequent for the natural period of the cell. Very rapid beating, as beats that are rapid enough to give rise

[27] *Ibid.*, p. 317.

to tones, must be mediated by fibers set into partial vibration; for in such cases fibers set into whole vibration will be too far apart to overlap. The beating in these cases may have a period more natural to the cell thus affected through partial vibration than is that of the objective tone. In such a case the tone that is mediated is a difference tone.[28]

Somewhat inconsistent with this view is the one expressed on the previous page of his book, where Ebbinghaus seems to take the view that beats are experienced immediately from the form of the objective complex wave. This is, of course, a different consideration altogether, and such response to non-pendular forms of the objective wave requires in the fibers a condition that is directly opposed to such delicate elasticity as partial vibration requires. The above explanation was on the basis of partial vibration by means of the formation of nodes. Yet Ebbinghaus says this: 'The above tone waves *a* and *b* will . . . interfere with each other, *i. e.*, the amplitudes of their individual vibrations, and therewith their intensities, will periodically strengthen and weaken. These fluctuations . . . differ very much according to the intensity and pitch relations of the two tones. Under certain conditions they have the rhythm $h - t$,[29] under others $2t - h$, and so on. The small structures of the basilar membrane vibrating sympathetically should not, however, be thought of as structures of the nature of tuning forks,[30] as has actually been done since the Helmholtzian theory. But, although they are set into vibration by external impulses only when these impulses in a measure correspond to their own vibration numbers, they have, doubtless, still only slightly elastic power and *can continue their movements no considerable time after the cessation of the objective impulse.* They can, therefore, on account of their acquired disposition, . . . not remain unaffected by external impulses of their own period; and, accordingly, the above mentioned amplitude

[28] *Ibid.*, p. 323.

[29] h = higher tone, t = lower tone.

[30] Yet on p. 321 he says as proof of the possibility of partial vibration of the basilar membrane fibers that 'the deeper fibers of the contra octave of the piano . . . produce their twelfth, yea even their fourteenth partial tone as a most splendid after-clang of the tone.'

fluctuations of the objective tone waves will be taken up more or less faithfully by the portions of the basilar membrane on the whole corresponding to them. If they recur relatively slowly we hear them as beats; in case of more rapid frequency, as rattling; with still greater frequency as roughness.'[31] And this roughness, of course, on greater frequency merges into a difference-tone.

There is, therefore, at the basis of Ebbinghaus's 'explanation' a fundamental conflict. However Ebbinghaus may attempt to explain the intertone, he must posit for interference a high degree of inelasticity of the basilar membrane fibers. Such an assumption, though, is directly contradictory to one of his main presuppositions, that of partial vibration, which requires highly elastic fibers in the cochlea. His assumption of partial vibration of the basilar membrane fibers for the explanation of beats of wide intervals, is likewise contradicted by that of inelasticity to explain how beats can give rise to difference tones.

We shall turn back now to the consideration of a few more recent experiments on the so-called interruption and the variation tones. It will be recalled that those writers—Koenig, Hermann, Wundt, Ebbinghaus—who had been offering theories to replace or to supplement the Ohm-Helmholtzian view had all conceived of the interruption tone as inexplicable on that view. It was to them an evidence that beats when frequent enough generate tones.

In 1901 K. L. Schaefer and Otto Abraham[32] took up the study of 'interruption tones' with a view to testing whether they existed objectively as pendular vibrations. To interrupt the tone they used the method employed by Dennert, that of stopping up certain holes of a siren. A large wooden disk 4 mm. thick and 15 cm. in diameter was used. It was perforated by a circle of holes 5 mm. in diameter. Of these 44 were tightly closed in such an order that 4 open holes alternated regularly with an equal number of closed ones. The disk was rotated with a constant speed by an electric motor and a cur-

[31] Ebbinghaus, *op. cit.*, p. 322.

[32] 'Studien über Unterbrechungstöne,' *Pflüger's Archiv*, LXXXIII., 1901, pp. 207–211.

rent of air was blown through a tube upon the circle of holes. A resonator was held to the circle of holes at the opposite side and connected with the ear of the observer. The experimenters found that whenever the 'interruption tone' corresponded to the pitch of the resonator it was very perceptibly reinforced. Professor Stumpf was present to witness the phenomenon and was convinced that the 'interruption tone' was objective. They used an ordinary cylindrical resonator for the purpose. When the vibration frequency of the interruption tone equaled 300 the experimenters reinforced the tone with a wooden resonator for the 300 fork. The first overtone of the 'interruption tone' was also reinforced, so they concluded that this tone is not simple but is a clang.

Koenig had also produced tones from disks with holes periodically variable in diameter. As the rotation increased the beats from these holes increased and finally produced a continuous tone. Dennert had obtained similar results. Now, Schaefer and Abraham also reinforced this sort of 'interruption tone' with resonators, thus proving that it, too, is objective. *The tone was unmistakably reinforced.*

These results, conclude the experimenters, prove that the so-called interruption tones, whether produced by actual periodic interruption or simply by periodic strengthening of a tone, are objective; *they correspond to actual pendular vibrations existing in the air.* Our perception, then, of these tones is in no way contradictory of Ohm's law. The tones exist as physical facts, which are perceived as any objective tone is perceived.

In another series of experiments[33] Schaefer and Abraham found that a disk with a circle of 60 holes varying periodically (five times) according to this scheme (where o = open hole, x = closed hole).

o o o o o o x o x x o x

also gave an 'interruption tone' reinforceable with a resonator. In this case they obtained, when the tone 60 corresponded to c^4,

[33] 'Studien über Unterbrechungstöne,' *Pflüger's Archiv,* LXXXV, 1901, pp. 536–42.

the 'interruption tone' $5 = f$. In four similar experiments they varied the scheme respectively as follows:

o o o o o o x x o o x x
o o o o o o x x x x x x
o o o o o o x o x o o x
o o o o o o x o o x o x,

and in each case got results similar to the above, *i. e.*, besides the tone 60 ($= c^4$) also the 'interruption tone' 5 ($= f$) which in every case was reinforced with a resonator.

Using a toothed siren, they obtained 'interruption tones' reinforceable to a still higher degree. The first siren of this kind that they used was 9.5 cm. in diameter and had on its edge 180 teeth equally distant from one another. When the disk was rotated the teeth were lightly touched with a visiting card. Now when every ninth tooth was removed they heard, besides the principal tone 9, also one corresponding to 1. This, as stated, was strengthened by means of resonators even more than was possible with interruption tones from perforated disks. Using a siren of 100 teeth, of which every fifth was filed down, they obtained, when the principal tone was c^4, an 'interruption tone' a^1. This also was strongly reinforced.

They decided now to study Hermann's *phase changing tones* in a similar way. They found in the laboratory two of Hermann's toothed disks arranged to reverse the phase of the tone 24 times in every 180 vibrations. On the one disk a tooth was missing between every seventh and eighth space, so that two spaces fell together; on the other a space was missing between every seventh and eigth tooth so that two teeth occurred together. Now in these cases, as Hermann had already observed,[34] besides the tone 180, the one corresponding to 24 was also plainly audible. This latter is the tone Hermann thought was caused by the periodic phase reversals. *But Schaefer and Abraham found that it was strongly reinforced by means of physical resonators.* Various other cases of the supposed phase changing tones, cases that had been studied both by Hermann and by Meyer, were tested with similar results. 'From these

[34] *Pflüger's Archiv*, LVI., 1894, p. 490 f.

results, which agree throughout, it may be concluded,' say Schaefer and Abraham, '*that the change of phase in and of itself gives no occasion for the generation of a particular sort of tone;* and that in those cases (first observed by Hermann) where a phase changing principal tone is accompanied by a second tone whose vibration-number corresponds to the number of phase reversals, the latter tone is to be regarded as a simple 'interruption tone' (dieser letztere als einfacher Unterbrechungston zu betrachten ist).

Later these men took up further experiments along the same line.[35] They used first a paste-board disk (Pappscheibe) 0.5 cm. thick with a simple row of 24 holes of 2 cm. diameter. The resonator, which was held close to the circle of holes as the disk rotated, was connected with the ears of the observer by means of rubber tubes (Hörschläuschen) so that a very slight reinforcement could be detected. It was found that when the disk was rotated *without blowing the holes* it produced a weak tone whose frequency corresponded to the number of holes that passed the mouth of the resonator per second. This tone was audible even when it was as low as the contra-A; sometimes it was perceptible even to contra-E. It was easily reinforced by resonators. To distinguish these tones from 'interruption tones' produced when a rotating disk interrupts a tone, they called them disk tones (Scheibentöne). They have always, of course, the same pitch as the 'interruption tone' obtained when the holes are blown upon through a tube.

The experimenters found that the *variation* tones (corresponding to $n + m$ and $n - m$, when n is a tone interrupted m times) are very perceptible when the tone of a loudly sounding fork is interrupted by a rotating disk. They decided to test for the objectivity of these tones. The disk used in this case was aluminium, and was 1 mm. thick. It contained a circle of 86 holes each 1 cm. in diameter. The disk was rotated sometimes by hand, sometimes by electricity. For the generation of the tone to be interrupted by the disk, they used principally the Bezold-Edelmann series of forks. By means

[35] K. L. Schaefer u. O Abraham, *Studien über Unterbrechungstöne, Pflüger's Archiv,* LXXXVIII., 1902, pp. 475–91.

of movable weights these forks can be made to give a continuous series of frequencies from the lowest audible tone to that of the tone a^2.

The variation tones heard in this series of experiments are given in a table, page 483. From this table it appears that they never heard both variation tones at the same time, though this is not stated explicitly in the report, I believe. The higher variation tone is in general more easily perceived than the lower. The latter was never heard with forks below f^1, and neither one was heard with forks below *e*. The tones heard were reinforced by several kinds of resonators. As proof that these variation tones reinforced were not 'disk' tones the experimenters found that they were strengthened *only when the fork,* held at the opposite side of the disk, *was directly opposite the mouth of the resonator.*

In view of these facts Schaefer and Abraham suggest the hypothesis that the so-called interruption tone is really a difference tone of the intermitted tone and one or both of the variation tones. The formulæ

$$(m + n) - n = m$$
$$n - (n - m) = m$$

show that it may possibly be a resultant of the coincidence of two difference tones. This idea, as the experimenters acknowledge, is not entirely original with them. For before Koenig had interpreted it as an interruption tone, A. M. Mayer, hearing this low tone, suggested that it might be regarded as "a resultant sound formed by the union of the sound of the fork with the upper and the lower of the secondary sounds,"[36] *i. e.,* of the variation tones. Difference tones, it will be remembered, were then frequently called *resultant* tones.

Koenig[37] had remarked that 'interruption tones' are weak with low forks, while with high loud forks, where the variation tones are scarcely, or not at all audible, they are very prominent. The observations of Schaefer and Abraham agree with the results of Koenig.[38] The intermittence tones for Koenig were

[36] *American Journal of Sciences and Arts,* CIX., 1875.

[37] *Quelques Expériences d'Acoustique,* pp. 138–140.

[38] K. L. Schaefer in *Nagel's Physiologie des Menschens,* III., 1905, p. 534.

very strong when the ratio of the frequency of interruption to the frequency of the given tone was 1:16 and 1:32, where the tones c^4 and c^5 were used. In these cases, as Schaefer and Abraham suggest, the variation tones (15, 17 and 31, 33) would lie so near the tone of the fork as to be difficult of discrimination from it. Moreover, the two primary tones in such cases would form intervals very favorable for the formation of loud difference tones. The experimenters never perceived the interruption tone with the fork c^2 as distinctly as Dennert[39] reports himself to have done. Their observations agree more closely with those reported by Koenig. 'The interruption tone was best obtained when forks of so high a pitch were used that the variation tones were hardly or not at all perceptible.'[40]

Even before Schaefer and Abraham published their experiments on the interruption tone, Zwaardemaker[41] described an experiment of his own, intended to throw light on this phenomenon. He purposely diverged from the ordinary method of interruption of the tone by means of the perforated disk, and connected a Blake microphone with a couple of Leclanche's elements in the primary coil of a small induction spool. The secondary circuit was opened and closed 64 times per second by means of an electrically driven tuning fork. In this secondary circuit was a telephone which was held to the observer's ear. If the circuit was closed and the tone of the fork conducted through a pipe to the microphone, it was plainly audible to the observer. Then when the tone was allowed to be interrupted 64 times per second a powerful interruption tone was heard, says Zwaardemaker. The interruptions themselves when no tone was conducted to the microphone made only an ill-defined noise. Zwaardemaker thinks that there is generated an objective vibration corresponding to the 'interruption tone,' and that it is a matter not for physiology but for physics to deal with these tones. He seems not to have tested for the objectivity of these tones, however.

[39] Dennert, *op. cit.*, 177 f.

[40] *Pflüger's Archiv*, LXXXVIII., pp. 486–7.

[41] H. Zwaardemaker, *Ueber Intermittenztöne.* Englemann, *Archiv f. Physiologie*, Sup. Bd., 1900, pp. 60–7.

The results obtained by Zwaardemaker, apparently reacting somewhat slowly on Schaefer and Abraham, called out from them an extended series of experiments published four years later.[42] They desired to test more accurately the intensity relations of the 'interruption tone' and of the principal tone, under conditions similar to those described by Zwaardemaker. In their experiments they were able to vary within wide limits the pitch of the principal tones and the frequency of interruption. They used tones of as high pitch as 4,800 vibrations. From more than 200 different tests in which the tones and the interruptions vary within wide limits, both absolutely and with respect to each other, they obtained the following results: The primary tone, which was heard very distinctly in the telephone when not interrupted, weakened very much or entirely disappeared as soon as the interruption process began. On the other hand, the more or less complicated clang appeared in its place. This latter clang contained one or two characteristic partial tones whose vibration numbers were dependent upon the pitch of the given principal tone and upon the frequency of the interruption. Under special conditions a confused tone was heard whose frequency equaled that of the interruption. These results seem to show that Zwaardemaker had not described the phenomena carefully enough. The tones obtained were in every case reinforced by resonators.[43]

Schaefer and Abraham conclude—as indeed Zwaardemaker, with far less support from experiment, had done—that the so-called interruption tone exists objectively as a pendular vibration capable of affecting physical resonators; that its explanation, therefore, is not a physiological problem. This tone consequently offers no difficulty to the Ohm-Helmholtzian law of tonal analysis. It is perceived upon the same principle as that upon which other objective tones are perceived.[44]

[42] 'Zur Lehre von den sogenannten Unterbrechungstönen,' *Drude's Annal. der Physik,* XIII., 1904, pp. 996–1009.

[43] *Ibid.,* p. 1000, and Nagel's *Physiologie,* III., 1905, p. 536.

[44] From their various statements they seem to consider the interruption tone as partly a 'disk tone' and partly a difference tone of two sets of primaries so closely connected as to make it exist objectively. Both of these constituents of the 'interruption tone' then are *objective.*

SECTION 5. INTENSITY RELATIONS.

We come next to one of the most difficult of acoustical problems. It is the question of ***intensity relations*** which we must now consider. It is a long story, but we must be brief. We shall take up, in connection with this question, the other troublesome one as to *what combination tones are actually heard*. The two problems are closely allied. This is evident if we admit—what may become clear soon—that there are combination tones which are not heard under normal conditions.

The earlier writers paid but little direct attention to the intensity relations of combination tones. Until Helmholtz's time only three or four difference tones had been heard.[1] In Helmholtz's work it is not always clear whether he actually heard the tones of which he writes. In his *Sensations of Tone*, page 155, *e. g.*, he considers difference tones up to the sixth 'order,' inclusive. In one place he tells us explicitly that he heard, from siren tones, not only the first summation tones but also those represented by $2p + q$ and $2q + p$.[2] These last, he says, were very weak. That summation tones in general are weak, he states both as the result of actual experience and in connection with his theoretical deduction.[3] He states explicitly that 'multiple' combination tones cannot as a rule be distinctly heard, but that in certain cases they make themselves known by beating with other tones.[4]

In 1876 A. M. Mayer made the important discovery that sounds of considerable intensity, when heard by themselves, may be completely obliterated by lower sounds of sufficient intensity. On the other hand he found that "no sound even when very intense, can diminish or obliterate the sensation of a concurrent sound which is lower in pitch."[5] This phenomenon, now well known, affects not only Ohm's law but also some

[1] Cf. Hällström, *supra*, p. 3.

[2] *Supra*, p. 23.

[3] *Sensations of Tone*, pp. 155–6; p. 413. Mathematics can, indeed, not yet be applied to the determination of the *relative intensities* of combination tones; it is of use, however, in the determination of the *frequencies* of such tones.

[4] *Ibid.*, p. 154 d.

[5] A. M. Mayer, 'Researches in Acoustics,' *Phil. Mag.*, 5th Series, II., 1876, pp. 500–7.

objections urged against Helmholtz's theory of combination tones.

We have already considered Koenig's results as to the question of what combination tones (beat-tones) are audible. While he did not raise the intensity question so directly as it has since been raised, he frequently spoke of the relative intensities of the 'beat-tones' and endeavored, it will be recalled, to explain the more easily perceptible summation tones as resulting from upper partials.[6] Later he altogether abandoned a belief in the existence of combination tones.

In 1894 A. M. Mayer reports an interesting experiment which he performed with bird-call whistles. These whistles gave tones beyond the upper limit of audibility: "With these whistles," he says, "beat-tones [*i. e.*, difference tones] have been obtained when the vibrations of either whistle alone were inaudible.

"Beat-tones," he adds, "have also been obtained by Dr. Koenig and myself in Paris with tuning forks whose frequencies surpass the limit of audibility. Dr. Koenig anticipated me in the production of these beat-tones by several months."[7]

Hermann in 1891 definitely raised against the Helmholtzian theory the objection not only that difference tones were in many cases heard louder than they should be according to that theory, but also that difference tones of the 'second order' were in some cases much louder than those of the first. He cites the case of the major third $c^2:e^2$ which gives the difference tone g^1 (= $2c^2 - e^2$) very distinctly. This one case alone, he says, is sufficient to disprove the validity of the Helmholtzian theory of combination tones.[8] Stumpf had indeed shortly before this observed the same phenomenon. He found that g^1 comes out more clearly when c^2 and e^2 are sounded in a pianissimo than it does when they are sounded loudly.[9]

[6] *Supra*, pp. [illegible] ff.

[7] Alfred M. Mayer, 'On the Production of Beat-tones from Two Vibrating Bodies whose Frequencies are so High as to be Separately Inaudible.' *Rep. of the Brit. Ass.* [illegible] 1894, p. [illegible]

[8] *Supra*, p. [illegible]

[9] [illegible] II, 1890, p. 24[illegible]. It must be noted, however, that on louder sounding of c^2 e^2 the first difference tone becomes so loud as to interfere with g^1 on the principle discovered by Mayer that lower tones may obliterate higher ones.

For the perception of difference tones, he concludes, it is not at all necessary, as Helmholtz had supposed, that the primaries be intense. Stumpf had not at this time abandoned the Helmholtzian theory. Indeed, as he suggests,[10] the second difference tone, according to Helmholtz's mathematical deduction, is not dependent upon the first, even though Helmholtz himself considered it so in the body of the *Tonempfindungen*.

Hermann was never able to hear summation tones. These according to Helmholtz's theory should be about equally strong with the difference tones, he says. Stumpf, rather inconsistently with the view he then held, was inclined to adopt the explanation of summation tones first offered by Röber, in 1856, and later suggested independently by Appunn to Preyer. This view, it will be recalled, regards the summation tones as difference tones of the second order, thus:

$$2h - (h - t) = h + t.$$

Stumpf says that series of observations on the harmonium, the siren, and different pipe instruments have led him to the conclusion that summation tones are strongest when there are strong over-tones present.

Since Meyer's theory arose largely from the fact that no other theory sufficiently explains certain of the phenomena under consideration, we may briefly consider it here.[11]

In his most recent statement,[12] Meyer divides combination tones into three classes, as follows:

1. Subjective.

2. Objective I., tones which arise under conditions represented by a harmonium or polyphonic siren, where there is a common wind chest.

[10] *Tonpsychologie*, II., note 3, p. 250.

[11] Professor Meyer informs me that he is about to publish, in English, a complete statement of his theory, so I shall not attempt a full description of it here. Since the note of the previous sentence was written the promised monograph has appeared. "An Introduction to the Mechanics of the Inner Ear," by Max Meyer, *The University of Missouri Studies*, Vol. II of the Science Series, No. 1. As the monograph is only an 'introduction' it does not enter as fully as was hoped into the questions of intensity relations. Moreover it is theoretical rather than experimental.

[12] Über Kombinations und Asymmetrietöne,' Drude's *Annal. d. Physik*, XII., 1903, pp. 89–92.

3. Objective II., tones that arise from the fact that an unsymmetrical body is forced to vibrate synchronously with two or more wave-series.

So far as I see, Meyer practically agrees with Helmholtz's explanation (with certain necessary modifications) of cases (2) and (3).[13]

He admits that summation tones are sometimes heard in these two cases but *not* in case (1). Both kinds of objective combination tones are of little interest to the psychologist, he says. Meyer's so-called wave-reduction theory, then, applies properly only to the group of combination tones usually called subjective. This group of subjective combination tones evidently includes the prominent combination tones which have no corresponding objective pendular vibrations,—those whose intensity Helmholtz's theory is supposed incapable of explaining. This makes possible a good deal of ambiguity as to which group shall claim certain tones ordinarily admitted to be 'subjective.' All weak tones which this theory cannot explain can easily be given over to Helmholtz, *i. e.,* to groups (2) and (3) of the divisions just given.

Meyer's theory, though it attempts to explain the origin of only the 'subjective,' combination tones, is, however, a theory of how the ear analyzes tonal clangs. All tones originating in the middle ear, or externally to the ear altogether, must come to the inner ear as 'objective.' They must, therefore, all be treated alike. The principle of his theory is this: When a wave impinges on the ear the movement of the stapes corresponds in general to the objective form of the wave, whether it is a complex wave or not. This produces certain forced movements of the whole organ of Corti and basilar membrane. The membrane is crowded downward near the stapes to make room for the liquid displaced by the inward bulging of the *fenestra.* The extension of the movement is, of course, dependent upon the force exerted against it and for the period of time that this force acts. Hence in the case of low tones it extends farther up the cochlea than in the case of high tones. In the former

[13] It must be noted, however, that he absolutely rejects Helmholtz's theory of resonators in the ear. The process of analysis of the tones that come into the inner ear is *entirely different* for the two theories.

case the inward bulging lasts longer. Now if a high tone and a low one operate at the same time the lower portion of the organ of Corti will evidently be stimulated more frequently than the portion farther up the cochlea, which is affected only by the less frequent vibration. Whenever the two wave-series act upon the *fenestra* in the same direction at the same time the resulting inward motion will be greatest and the stimulation of the organ of Corti will extend farthest up the cochlea. Different sections of the organ will consequently be stimulated with different periods of frequency according to the form of the complex wave that is affecting the ear. Now, *pitch is determined entirely by the frequency of the stimulation of the organ of Corti.* Each section of this organ, therefore, that is stimulated will mediate a tone corresponding to the frequency of its stimulation.

It is to be noted here that Meyer's theory affords an easy explanation of the fact, which probably no other existing merely physiological theory can explain; viz., that a high tone may be obliterated by a lower one, whereas the opposite is not true. A low tone though very weak cannot be obliterated by a higher tone, even of great intensity.

By a very laborious process, partly determination of the form of the objective wave, and partly mathematical calculation, Meyer has endeavored to find, in the case of several given intervals, what tones *ought* to be heard, and what should be the amplitude and frequency relations of them.[14]

The results are not very satisfactory. In the interval of the fifth, *e. g.*, where the primaries have equal given amplitudes, the calculated results would make the difference tone (1) most intense, the lower primary (2) next in intensity, and the upper tone (3) comparatively very weak. It is, of course, but just to say that the relative intensity of tones cannot be determined solely by the ratios of their amplitudes. In reply to the charge that the tones actually heard do not correspond exactly with his calculated results Meyer suggests that a consideration at the same time of *all* the various aspects of the complex process is impossible; that results can be obtained from the consideration of only one of the variables at a time, to the

[14] See especially *Zeitschr. f. Psychol.*, XL., 1896, 177 ff.; and XVI., 1898, 22 ff.

neglect of the others. Very well, Helmholtz's mathematical calculations are made under very similar conditions, as Helmholtz himself admits. Without considering then the question as to whether Meyer's theory is superfluous, and as to whether his tri-partite division of combination tones is unnatural—such questions will be more in place when we shall have seen his new book—let us consider his laws for subjective combination tones.[15]

SECTION 6. Meyer's Laws of Combination Tones.

These laws "do not express *all* the difference tones which one might possibly hear in every possible combination of objective tones, but merely those differences [*i. e.,* difference tones] which one is *most likely* to hear in those combinations which correspond to relatively simple ratios."

1. When the ratio of the vibration rates does not differ much from 1:1 (*e. g.,* 11:12 or 9911:9989) only one difference tone is heard. It is expressed by the formula $h - t$, where h is the higher tone and t is the lower. In this case a "mean" or "intertone" is also heard, as described by Stumpf.

2. When the ratio of the frequencies is of the form $n:n+1$ the difference tone corresponding to 1 (*i. e.,* $h - t$) is always strongest. A few of those also appear whose numbers correspond to $n - 1$, $n - 2$, etc.; *e. g.*, the tones 8:9 gives 1 and 7, 6, 5. If n is a rather small number, we really hear *all* the tones from n down to 1; *e. g.,* the tones 4:5 give 3, 2, 1.

3. Other ratios of small numbers, representing intervals less than an octave, give combination tones represented by $h - t$, $2t - h$, $2h - 3t$. If the interval is less than the fifth, $h - t$ is strongest; if it lies between the fifth and the octave, $2t - h$ is the strongest.

4. Intervals larger than the octave do *not* give the first difference tone ($h - t$) which would be between the primaries. As a rule only one difference tone is easily noticeable in these cases. It is found by taking the "*smallest* difference between the larger number of the ratio and any multiple of the smaller number," *e. g.,* the tones 4:11 give $3 \times 4 - 11 = 1$.

[15] Cf. 'Auditory Sensations in the Elementary Lab. Course,' *Am. Jour. of Psychol.,* XVI., 1905, 293–301; *Zeitschrift,* XVI., pp. 2–3.

Now, as is easily shown from objective curves, any change in relative intensity of the primaries will not only make a difference in the relative intensities of the difference tones, but may even result in the appearance of new difference tones, or in the disappearance of some such tones which before the change were audible. Empirical facts here agree with theory. But Meyer has grave difficulties to meet in such cases, as did Koenig also, from the fact that determinations from the objective waveform frequently indicate that a primary tone should have far less intensity than one or more of the difference tones, when such is not actually the case in hearing.[1] Meyer, of course, appeals to complexity of variables.[2]

In view of difficulties of this kind it is questionable whether Meyer's theory is an improvement upon that of Helmholtz even with respect to the intensity difficulty. And this is the very thing Meyer's theory was devised primarily to explain. Certainly the burden of proof of the superiority in this respect is on the *new* theory. It is true that on account of the " piecemeal " way in which it has been written, Meyer's theory has often been unjustly criticized. It remains to be seen how it will acquit itself of these difficulties when it appears in a single book.

But there are yet other difficulties. Meyer's theory has no place for summation tones and for difference tones lying between the primaries. Krueger in a series of very careful experiments with well-trained subjects, has proved that both of these kinds of tones can actually be heard with suitable intervals. The perception of them of course requires careful dis-

[1] Cf. Meyer's own curves in *Zeitschrift für Psychol.*, XI.

[2] " I think it is often overlooked in discussions on this subject," says Lord Rayleigh, " that a difference tone is not a mere sensation but involves a *vibration* of definite amplitude and phase. The question at once arises, how is the phase determined? It would seem natural to suppose that the maximum swell of the beats corresponds to one or other extreme elongation of the difference-tone. . . . Again how is the amplitude determined? The tone certainly vanishes with either of the generators. From this it would seem to follow that its amplitude must be proportional to the product of the amplitudes of the generators, exactly as in Helmholtz's theory. If so, we come back to difference tones of the second order, and their asserted easy audibility from feeble generators is no more an objection to one theory than to the other." *Theory of Sound,* Vol. II., 1896, p. 462.

crimination. Meyer has indeed heard these intermediate difference tones himself (*e. g.*, 5 from the intervals 4:9 and 3:8).[3]

In his review[4] of Schaefer's theory of subjective combination tones, however, I understand him to say that *subjective* difference tones lying between the primaries do ***not exist***,[5] and that subjective summation tones are explicable as difference tones. Perhaps the various apparent contradictions on these points are due to his use of the term 'subjective.' Whatever stand he may take as to subjective summation tones and intermediate difference tones, they can apparently not be accounted for on the principle of his theory.

A few years ago, Krueger,[6] 'independent of any bias as to theory,' undertook an extended investigation of the phenomena resulting from two simultaneously sounding tones of intervals varying from $n:n$ to $n:4n$. He used tuning forks with adjustable weights. The tones were conducted through pipes from the resonance boxes of the forks to an adjoining room where the observer was seated. Upper partials were eliminated with considerable success by means of a number of side pipes perpendicular to the main one. The length of each pipe was one-fourth that of the wave of the overtone which was thus to be eliminated by interference. The forks were actuated with as much uniformity as was possible. The ground tone was usually c^1 (256) or one of its octaves c^2 or c^3. 'Occasional trials with other ground tones lead to no perceptibly divergent results.'

Krueger had *nine well trained observers*, one of them being a violinist with very acute analytic powers for high tones. Occasionally Krueger himself served as observer.

The article is very long. Only a few of the most important of the results can be given here. Krueger divides the intervals studied into three periods. The first extends from $n:n$ to $n:2n$, the second from $n:2n$ to $n:3n$, the third from $n:3n$ to $n:4n$.

Summation tones were studied only incidentally, *i. e.*, the

[3] *Zeitschrift für Psychol.*, XI., pp. 186–7.

[4] *Pflüger's Archiv.*, LXXXI., 1900, p. 56.

[5] Kreuger so understands him too. Cf. *Phil. Stud.*, XVII., 1901, p. 205 note.

[6] Felix Krueger, 'Beobachtungen an Zweiklängen,' *Phil. Stud.*, XVI., 1900, pp. 307–379 and 568–663.

observers were not asked to study them, but frequently called attention to them. They were heard, however, in all three periods, but were loudest in the first where the interval is less than an octave. The violinist who served as subject, was able to hear them at almost any time. The most favorable interval for the hearing of summation tones proved to be in the proximity of the major seventh. Krueger feels sure that these tones are really *summation* tones, for the primaries proved to be practically free from overtones. The pitch was determined by means of a tonometer. Occasionally (as was found to be the case with difference tones as well) the summation tones would seem to be 'subjectively raised or lowered.' In the third period (intervals between $n:3n$ and $n:4n$) the summation tones were too high for the tonometer, and hence could not be accurately measured. Dr. Möbius, the violinist, frequently called attention to them even here. Summation tones as a rule appeared towards the end of the clang as if in the period of loudest sounding of difference tones they had been obliterated.

Low difference tones were, in general, heard earlier in the clang, and for a shorter time, than higher ones. In both of the periods in which the intervals were greater than an octave (*i. e.*, $n:2n$ to $3n$ and $n:3n$ to $4n$), the first difference tone was heard. This difference tone, it will be noted, was *intermediate in pitch between the primaries.* Stumpf has recently acknowledged that he is convinced of the existence of intermediate difference tones.[7] These tones are always very weak in intensity. The results of Krueger's experiments show that from the compound clang of two simple tones there result, besides the summation tone, about five difference tones of different orders whose pitches may be determined from the following rule: Find first the difference of the vibration numbers of the primaries; then continue to find the difference between the two smallest numbers resulting after each successive subtraction. The series of differences obtained represents the difference tones.[8] Representing the first difference tone by D_1, the second by D_2, etc., Krueger gives the following formulæ for the determination of the pitch of all but D_5:

[7] *Zeitschr. f. Psychol.*, XXXIX., 1905, p. 268.

[8] F. Krueger, *op. cit.*, pp. 22–3.

$D_1 = n' - n$ (n' being the higher primary tone).

$D_2 = \pm (n - D_1)$.

For intervals smaller than the octave:

$D_2 = n - D_1 = 2n - n'$.

$D_3 = \pm (D_2 - D_1) = \pm (3n - 2n')$; or

$= D_2 - D_1$, when the interval is less than the fifth, and

$= D_1 - D_2$, when the interval is between the fifth and the octave.

$D_4 = \pm (D_3 - D_1) = \pm (4n - 3n')$ for intervals up to the fifth; or

$= D_3 - D_1$ for intervals smaller than the fourth, and

$= D_1 - D_3$ for intervals between the fourth and fifth.

$= \pm (D_2 - D_3) = \pm (4n - 3n')$ between the fifth and the octave, or

$= D_2 - D_3$ for intervals between the fifth and the major sixth, and $D_3 - D_2$ for intervals between the major sixth and the octave.[9] In no case was the difference tone $2D_1$ of the first over-tone heard.[10]

Intertones (Zwischentöne) were frequently observed. This phenomenon occurred not only between primary tones of small intervals but also between a primary and a difference tone, or between two difference tones. The intertone, as Krueger describes it, does not seem to be so definitely and distinctly a *tone* as one would suppose from Stumpf's description.[11]

Helmholtz's theory had been objected to on account of the fact that it could not explain the beats of wide intervals, such as Koenig had observed. While Stumpf early observed these phenomena which Koenig had described, he remarked that there are two kinds of beats easily distinguishable to the practiced ear,—beats of the higher primary tone, and deeper beats connected with the lower tone.[12] When overtones of the pri-

[9] *Ibid.*, pp. 326–7.

[10] *Phil. Stud.*, XVII., 1901, p. 222.

[11] In my own introspections it is always, more or less, a beating mass and the primary tones are drawn somewhat nearer together. If one of the primaries is suddenly stopped, the other at once makes a little 'jump' to its normal pitch. Mr. Bingham, who has a well-trained ear, tells me that his experience is similar to mine in this respect.

[12] C. Stumpf, 'Ueber die Ermittel. von Obertönen,' *Wied. Annal.*, LVII., 1896, p. 660 ff.

maries were present both kinds of beats were perceptible, but when, by means of interference, the upper partials were eliminated the beating of the upper primary tone disappeared while that of the *lower tone remained unaltered.* Stumpf explained the upper beating as due to interference of overtones of the lower tone with the upper primary, and the lower beating he supposed to arise from the interference of the lower primary tone with a difference tone of nearly the same pitch. Meyer reports that both these sets of beating (*i. e.*, upper and lower) disappear when the first overtone of the lower primary was eliminated through interference.[13] Meyer says his tones were weakened considerably in passing through the interference pipes for the elimination of the overtones. Krueger suggests[14] that this probably explains the difference. Meyer's primaries were weakened so much that difference tones did not exist. Krueger, from his extended experiments with tones freed from upper partials by a better interference method, finds it possible to account for all the beats of wide intervals, not due to overtones, by the presence of difference tones which either beat with the lower primary tone or with each other. As upper partials were gradually removed by interference, he found that the beating with the upper primary gradually disappeared. Krueger thinks that his experiments remove from the 'beat-tone' theory its most valuable support.[15]

Krueger thinks that his results indicate that there are no subjective overtones, such as might be expected from Helmholtz's mathematical theory. This, it seems to me, however, cannot be urged with much force. These overtones would be very weak and would be hard to discriminate from the generating tones (an octave below).

While the Helmholtzian theory affords an easy and simple explanation of various pathological cases, which need not be described here, it is well known that it has proved insufficient for the explanation of the so-called subjective combination tones. Dennert[16] and others have reported cases in which such

[13] *Zeitschr. f. Psychol.*, XVI., 1898, 9 f.

[14] *Phil. Stud.*, XVI., p. 233.

[15] *Ibid.*, p. 246.

[16] *Arch. f. Ohrenkeilk.*, XXIV., 1887; also Nagel's *Physiol. des Menschen*, III., 1905, p. 569.

combination tones have been heard by subjects who had lost from both ears the tympanic membrane and the first two ossicles. I have myself, with Mr. Bingham,[17] studied such a case in which the subject readily heard both the *first* and the *second* difference tones of tuning forks. While it is true that cases of this kind do not prove that Helmholtz's theory is *wrong*, they, at any rate, show that subjective combination tones may be due to causes other than asymmetry in the drum. Helmholtz's explanation of the origin of such tones is, to say the least, incomplete.

In recent years K. L. Schaefer[18] has proposed a supplement to the Helmholtzian theory of subjective combination tones. Helmholtz had made a very simple mathematical statement concerning the origin of *objective* combination tones when the primary tones were produced by generators with a common windchest (*e. g.*, the polyphonic siren). He admitted that his treatment of the case was very imperfect.[19]

A complete statement of the conditions would give more resultant tones than those obtained. The essential condition for the generation of such resultant objective tones is this. There must be exerted on the air escaping through one of the periodically opening and closing holes of the wind chest, a periodic change in pressure due to the escape of air through the other hole. Now Schaefer sees in the inner ear a condition fulfilling this requirement and analogous to the case of the siren, when the tones m and n fall on the ear the movement of the stapes against the oval window may, for practical purposes, he argues, be conceived as equal to two such organs operating separately against that window. Conditions similar to those in the wind chest of the polyphonic siren will then be produced in the liquid of the inner ear. There will be a periodic amplitude fluctuation of the 'vibrating bodies' set into motion by the primary tones. This will give rise to the combination tones

[17] Cf. W. D. Bingham, 'Rôle of the Tympanic Mechanism in Audition,' *Psychol. Rev.*, XIV., 1907, p. 229.

[18] K. L. Schaefer, 'Eine neue Erklärung der subj. Combinationstöne auf Grund d. Helmholtz'schen Resonanzhypothese,' *Pflüger's Archiv*, LXVIII., 1899, pp. 505–526.

[19] Cf. *supra*, p. 21.

$n + m$, $n - m$, etc., as is shown by Helmholtz's determination of objective combination tones.[20] As evidence in favor of his view he cites the rather close correspondence between the objective and subjective combination tones. He himself undertook an investigation of the first difference tone with intervals greater than an octave. Besides other instruments, he used for generating the primary tones both tuning forks and the harmonium; *i. e.*, he tested for the existence of both subjective and objective intermediate (zwischenliegenden) difference tones. In no cases were they audible. Even properly tuned resonators did not make audible the *objective* difference tones in question. He concludes from his experiments '*that both subjective and objective intermediate difference tones either do not exist at all, or that, as opposed to other difference tones, they are at least too weak to be perceived under the ordinary conditions of hearing.*'[21] Koenig and Meyer had heard difference tones intermediate in pitch between the primaries.[22] This Schaefer does not deny. He explains them, however, as being due to upper partials thus:

$$8 - 3\ [= 7 \times 3 - 2 \times 8] = 5.$$

He admits on page 520, that he himself with Professor Stumpf had heard the tone 3 as a difference tone from the interval 5 : 2. This tone, he says, is accounted for on the same principle, *i. e.*, $4 \times 2 - 5 = 3$. Summation tones when they have been heard, are to be accounted for also as difference tones resulting from the presence of upper partials.

Max Meyer in a review of Schaefer's article,[23] denies the alleged correspondence between objective and subjective combination tones. Rücker and Edser, he points out, had proved the objective existence both of summation tones and of inter-

[20] Recently Schaefer reports an experiment (in *Drude's Annal. d. Physik*, XVII., 1905, p. 572 ff.) in which he demonstrated that membranes vibrating to two tones give rise to objective combination tones. It is not clear whether he means to apply this to the inner membranes of the ear, as part of his explanation of subjective combination tones.

[21] *Pflüger's Archiv*, LXVIII., p. 512.

[22] Cf. *Pogg. Annal.*, CLVII., 1875, p. 194, also 216; *Zeitschr. f. Psychol.*, XI., 1896, pp. 186–7.

[23] *Pflüger's Archiv.*, LXXXI., 1900, 49 ff.

mediate difference tones when the primaries are produced from generators with a common wind chest. On the other hand, he says, the corresponding subjective combination tones do not exist (?). Meyer has elsewhere[24] expressed the conviction that subjective intermediate tones *are* audible. He thinks now that he was wrong.

Meyer ridicules the idea of analogy between the ear and the wind chest of the harmonium or siren. 'The Helmholtzian formula,' he says, 'is applied to a case here to which it is absolutely inapplicable (den sie absolut nicht passt).'[25] As to vibrating bodies in the air (from which Schaefer had drawn an analogy), Meyer says, 'Neither any theory nor any experience establishes the assumption that a body vibrating in a liquid (Flussigkeit) with the simple tone *m*, is in any perceptible way influenced by the pressure fluctuations due to the tone *n* in the surrounding medium. Much more, the general and well grounded conviction exists, that two bodies vibrating in the same fluid under normal conditions remain mutually uninfluenced.'[26] In his later article in the *Anal. d. Physik*, referred to above, Meyer mentions the experiment of Rücker and Edser as a positive proof against such an assumption. These experimenters, however, state explicitly that they lay 'less stress' on their negative than on their positive results. The resonating fork and mirror may not have been delicate enough for the detection of weak vibrations that may actually have been present. Of course Meyer's phrase "in any perceptible way" saves him here, *so far as experiments have gone.* Meyer's statement concerning theory, however, should be compared with the quotation from Lord Rayleigh on p. 65 above.

In reply to Meyer's review, Schaefer[27] admits that objective summation tones and subjective intermediate difference tones exist, but says that they are too weak to be heard under normal experimental conditions (üblichen Versuchsbedingungen). He

[24] 'Ueber Combinationstöne, etc.,' *Inaug.-Dissert.*, Berlin, 1896, p. 12.

[25] *Pflüger's Archiv*, LXXXI., p. 49.

[26] *Ibid.*, p. 54.

[27] *Pflüger's Archiv*, LXXXIII., 1901, 73 ff.

says also that the same thing is true of the corresponding subjective tones.[28]

We shall now consider the question whether the conditions of origin of *objective* and *subjective* combination tones are not really *in principle* the same, even though they are conveniently treated mathematically as different. For purposes of brevity we shall speak of the case of *objective* combination tones as case I., and of that of *subjective* combination tones as case II.

Helmholtz makes some statements that will be of use to us in this connection. (1) He admits that he has considered case I. only in its simplest aspect; that the complete development is very complicated and will result in the determination of combination tones of various 'orders';[29] and he tells us that he himself has heard even summation tones of the 'second order' from primaries generated with the siren.[30] (2) He reports, as the result of experiment, that even these supposedly objective combination tones are largely generated within the ear itself.[31] We shall see later that as carefully gathered empirical facts accumulate, the objective and subjective combination tones come more and more to a correspondence with one another. (3) It is admitted by Helmholtz,[32] and also by Lord Rayleigh, one of the best authorities on sound waves, that the motions of air and other elastic media admit of a treatment perfectly similar to that of case II., as found in Appendix XII., *Sensations of Tone*. In a reply to an objection urged by Hermann,[33] that Helmholtz's explanation of subjective combination tones depends entirely upon an assumed failure of symmetry, Lord Rayleigh says: "This objection . . . is of little practical importance, because the failure of symmetry nearly always occurs. It may suffice to instance the all important case of aerial vibrations. Whether we are considering progressive waves advancing from a source, or the stationary vibration of a resonator,

[28] Krueger's experiments proving that they are actually audible to trained observers, had already been published. *Phil. Studien*, XVI., 1900, p. 307 ff.

[29] Helmholtz, *op. cit.*, p. 420.

[30] *Supra*, p. 23.

[31] *Supra*, p. 21.

[32] *Helmholtz*, op. cit., p. 412 a.

[33] *Pflüger's Archiv*, XLIX., 1891, p. 507.

there is an essential want of symmetry between condensation and rarefaction, and the formation in some degree of octaves and combination tones is a mathematical necessity."[34]

This statement makes it all the more probable that the two cases developed by Helmholtz can both be worked out on the same principle of 'superposition of vibrations.' It seems to me that Helmholtz is not right in supposing that objective combination tones can be deduced from the siren even where the vibrations are *infinitely* small,[35] for, as he himself explicitly states, no combination tones will arise until there is "a second greater opening of variable size, through which there is a sufficient escape of air to render the pressure p periodically variable, instead of being constant." For the principle of superposition to take effect it is necessary only that the generating tones should somewhere be closely associated, and that at this place, wherever it happens to be, the amplitudes of their vibrations should have a finite ratio to the mass vibrating in common with the two tones. Now where such connection does not obtain externally to the ear, the conditions certainly are fulfilled *within* the ear. Even though we exclude all considerations of the membranes themselves, we find a favorable condition in the *fluids of the cochlea.* The objection that in these fluids the vibrations are very small is easily met by the fact that the mass of the vibrating structure (*i. e.*, the fluids) is also very small, so that the proportion still may easily be finite.

It is gratifying to note that this view is by no means contradictory to what Lord Rayleigh has to say. To quote: "The production of external or objective combination-tones demands the coexistence of the generators at a place where they are strong. [He adds in a footnote: 'The estimates for condensation of sounds just audible make it highly improbable that the principle of superposition could fail to apply to sounds of that order of magnitude.'] This will usually occur only when the generating sounds are closely associated as in the polyphonic siren and in the harmonium. In these cases the conditions are especially favorable, because the limited mass of air included

[34] Rayleigh, *Theory of Sound,* Vol. II., 1896, p. 459.

[35] See quotation from him, *supra*, p. 19.

in the instrument is necessarily affected by both tones,"[36] which is, of course, equivalent to saying that the proportion of the amplitude to the mass affected is a *finite* one.

Whether or not the two cases are physically one in principal—and we shall leave this question here with physicists—we may still, on the authority of the statements of Helmholtz and of Lord Rayleigh just referred to, apply the principle of superposition to the liquids of the inner ear, where the vibrations of the primary tones are in close relation. We may suppose that all subjective combination tones arise by this means. Let us see then what combination tones *might* be heard according to Helmholtz's determination given in Appendix XII., pp. 412–3, of *Sensations of Tone*. The second term (x_2) of the series for x gives

$$2p,\ 2q,\ p - q, \text{ and } p + q.$$

The third term (x_3) gives

$$3p,\ 3q,\ 2p \pm q,\ p \pm 2q.$$

This is as far as Helmholtz has carried the deduction. When it is carried farther the fourth term (x_4) gives

$$4p,\ 4q,\ 3p \pm q,\ 2p \pm 2q,\ p \pm 3q.$$

The fifth term (x_5) gives

$$5p,\ 5q,\ 4p \pm q,\ 3p \pm 2q,\ 2p \pm 3q,\ p \pm 4q;$$

and so on. In general the ith term (x_i) gives

$$ip,\ iq,\ (i-1)p \pm q,\ (i-2)p \pm 2q,\ (i-3)p \pm 3q \ldots \\ 2p \pm (i-2)q,\ p \pm (i-1)q.$$[37]

Helmholtz's assumption upon which he based his equation of motion is of course only an arbitrary one, but the results show that the hypothesis which explains combination tones as resulting from superposition of the primary tones accounts for tones which *may* have occasionally been heard by Koenig and others and which have been regarded by some of Helmholtz's opponents as inexplicable on the basis of his mathematical

[36] Lord Rayleigh, *Theory of Sound*, II., 1896, p. 459.

[37] I am indebted to Professor F. R. Moulton, of the University of Chicago, for the further integration of the Helmholtzian equation.

theory. It is, of course, to be expected that only a few of all of these resultant tones are at any time audible and that those heard will belong to the 'lower orders.' The fact that occasionally a second difference tone $(2q - p)$, *e. g.*, is experienced louder than a first ought not to be urged too strenuously against Helmholtz's theory, not, at any rate, until we know more of the exact structures in the inner ear which are concerned in this purely mathematico-physical statement. Periods within the most practiced or most usual range, *e. g.*, ought perhaps to be experienced as relatively louder than periods less frequently experienced. The second difference tone of the major third (4:5), *e. g.*, is considerably louder than the first *only when its pitch is near the middle of the ordinary scale.* This is at least true of the forks at my command. Pure mathematical treatment can be applied to the operation of anatomical structures only with caution. Any criticism of Helmholtz's theory of hearing, then, which is based on the failure of the theory to explain the intensity relations of combination tones as actually experienced must take account of such obvious facts as those here indicated.

When the vibrations are so large that the displacements affect the *fourth* power of x in the equation $k = ax + bx^2 + cx^3 + dx^4$, another series of tones will arise, some of which will coincide with some of those determined above where only the *second* power of x was considered.

PART II. EXPERIMENTAL.

The Explanation of Summation Tones as Difference Tones of Upper Partials Shown Impossible.

Wherever it has been practicable I have repeated the experiments reviewed in the foregoing part of the paper, not so much to verify the results—my purpose was not so pretentious—but to make more real to myself the conditions and phenomena under consideration.

I have not been able to hear Hermann's phase-changing 'middle tone.' The intertone described by Stumpf and others is to me not really a tone; it is more nearly a beating complex, involving, for small intervals, both of the primary tones. In such cases, when one of the primaries is suddenly stopped, the pitch of the beating complex makes a clearly perceptible shift to the pitch of the remaining tone. The intertone is by no means so clear to me as one would expect from Stumpf's description of it.

By putting a loop of a string round the stem of a tuning fork and tying the ends to two hooks in the ceiling, one can easily illustrate the beating phenomenon of the rotating tuning fork. In such a case the fork can be 'wound up' like a top and left to unwind while sounding. The beats are then clearly perceptible. By pulling on the strings one can increase the rate of rotation until the beats become very rapid. The easiest way, however, of producing the 'interruption tone' is probably to stop up the holes in an ordinary metallic siren-disk so that about three open holes alternate with three closed holes, and to blow through a tube upon these holes while the disk is being rotated. This, it will be recalled, was Dennert's method. To me the 'interruption tone' is usually more or less ill-defined. When the rate of rotation of the disk is very rapid this tone becomes less easily perceptible. This may be due to the noise of rapid rotation, a disturbance that is practically unnoticeable

on slower rotation. At a certain medium speed of rotation the 'interruption tone' is relatively more prominent. The intermitted tone is clearly audible even on very rapid rotation. This tone was always easy to locate because it was produced from one of the four circles of holes in the disk representing a major chord. A good way to hear what K. L. Schaefer calls the *disk tone* is this: Rotate rapidly a metallic disk perforated with a circle of large holes, holding the ear near the circle. Now and then touch the circle of holes with the corner of a piece of paper. The paper is, of course, set into vibration and produces a tone equal in pitch to the 'disk tone.' The former serves to locate the latter. The 'disk tone,' I found, is audible to the unaided ear. It is this tone which Schaefer regards as partly constituting the so-called interruption tone. I produced good 'interruption tones' with paste-board disks perforated with a circle of holes varying periodically in diameter, by blowing upon the holes (while the disk was in rotation) through a flat tube as wide as the greatest diameter of the holes. The 'interruption tone' thus produced is doubtless purely objective, as Schaefer has shown, the increase in the size of the holes being practically equal to an addition of other circles of holes of equal period as illustrated in the accompanying figure.

o O o O o O o O

A

o o o o o o o o

o o o o

B

It is possible that a disk perforated with a single row of holes periodically variable as shown in *A* will produce two tones similar to those of a disk with two circles of holes as represented in *B*, when both circles of the latter are blown upon through separate tubes.

I did not attempt to test the objectivity of the 'interruption tones,' feeling that I had not sufficient control of the speed to locate the pitch satisfactorily.

The experiment of Cross and Goodwin is easily repeated. I put a small piece of wax into each ear and touched each piece

with the stem of a vibrating tuning fork. When the forks were of nearly equal pitch the beats were clearly audible, even when the tones were very weak. The shift in the apparent direction of the sound, described by Lord Rayleigh,[1] seemed to me to be rather one of *emphasis*, or intensity, both tones being heard in their proper locations continuously. After the tones could no longer be heard to beat when the forks were held against the top of the head, or one against the wax in one ear and the other against the teeth, they were still heard to beat when held against the wax one in each ear. It is interesting to note, in this connection, that it is practically impossible for a low tone to obliterate a higher one when the tones are thus communicated separately through the wax in the ears. This result, if my observations are substantiated by other tests, is most easily explicable on the basis of a theory like Meyer's or that of Kuile's.

In my own case it took considerable practice to hear intermediate difference tones and summation tones from tuning forks, but I feel satisfied that I succeeded in both cases. Some of the other students in the laboratory heard the summation tone after very little practice.[2]

The important experiments *not* repeated in this work are (1) those, as has just been said, which were devised to prove the objectivity of 'interruption tones' (Schaefer and Abraham); (2) those devised to produce 'phase-changing tones'. (Koenig, Hermann, *et al.*) and those which proved that these tones are strengthened by physical resonators (Schaefer and Abraham); (3) those experiments which have established with certainty the objectivity of combination tones in cases where the primary tones are in close mechanical connection externally to the ear (Rücker and Edser, *et al.*); (4) elaborate tests on a great number of intervals—both consonances and dissonances—to determine the general laws for the occurrence of combination tones (Koenig, Meyer, Krueger) and (5) experiments, not yet carried out satisfactorily by any one, studying the intensity relations of combination tones.

[1] Note, p. 46, *supra*.

[2] See below, p. 125.

The historical treatment has been made as brief as it was thought well to make it in view of the fact that no adequate statement of results already obtained has hitherto been given in the English language. The experiments which follow are limited to the investigation of those explanations of the summation tone which make it simply a difference tone of some sort. In this limitation, the fact has not been overlooked that the problem of intensity relations is probably the most important of all the present day open questions in the field of acoustics. For two good reasons this problem is not taken up here. (1) The (Koenig) tuning forks of this laboratory, and possibly of any other laboratory, are inadequate for extended experiments bearing upon this question. Not only are they not variable in pitch to any extent, but different forks vary greatly in the quality (in the non-technical sense) of their tones.[3] And this is not all. Some of the forks seem to be stiffer and less easily actuated than others, and their tones disappear earlier. It is practically impossible, therefore, to secure any uniformity in the primary tones used. The various 'wind-instruments' are even more unsatisfactory in experiments on intensity. With such instruments it is next to impossible to get smooth feeble tones. (2) Max Meyer is about to publish, in English, a complete statement and justification of his own theory of hearing.[3a] This theory was devised principally to explain the intensity relations of combination tones where Helmholtz's theory is said to fall short. It is but fair, then, to leave to Meyer the intensity problem. This second reason also explains why, in the historical sketch, the question of intensity relations did not receive as full treatment as it merited.

The method followed in these experiments is largely that of detecting feeble tones by means of 'auxiliary tones,' as they were called by Koenig. This method was used with considerable confidence by Koenig,[4] and has also been used to some extent by Lord Rayleigh.[5] It seems to be discredited by Krueger

[3] See Dr. A. Wyczolkowska, 'A Study of Certain Phenomena Concerning the Limit of Beats,' *Psy. Rev.*, 1906, XIV., p. 378.

[3a] Cf. note 11, p. 91, above.

[4] See quotation, *supra*, p. 52.

[5] *Theory of Sound*, Vol. II.

who says that he 'never reverted to the deceptive method of beating auxiliary forks.'[6] This statement, it is true, is made in connection with the study of *intermediate difference tones.* In such cases the method is unquestionably useless, and even deceptive. *E. g.*, if the auxiliary tone 5^{-}[7] beats when the primaries 4 and 9 are given, we are not at all sure that the beats indicate the presence of a difference tone 5. As some of the experiments which follow suggest, we are, in this case, more likely to hear beats of a difference tone 4^{+} (*i. e.*, $9 - 5^{-}$) with the lower primary. Koenig's experiments with auxiliary forks are subject to criticism in this respect. In the following experiments, where the details of the methods used are explained more fully, the possibility of error here indicated, as well as others that will become evident, are carefully guarded against. There certainly are legitimate uses of auxiliary tones.

It is well known that when two or three given tones produce two difference tones of nearly the same pitch, both or all three (as the case may be) of the generating tones beat very plainly. The beating is in general most prominent when the interfering difference tones are both of the so-called first order. Krueger[8] lays much stress on the fact that, when upper partials are ruled out, the beating of mistuned consonances is due to the interference of low difference tones. Of course the primary tones may themselves beat when the interval is small. In the cases of imperfect consonances, however, one, at least, of the difference tones which interfere must be of an 'order' higher than that of the first. Suppose that the frequencies are 200 and 301. Here the first difference tone is $301 - 200 = 101$. The second is $2 \times 200 - 301 = 99$. Now $101 - 99 = 2$. Consequently two beats per second would be heard. When an interval like this is sounding so feebly that the difference tones are not heard, one invariably locates the beating in the *primary* tones. Even when the first difference tone of the imperfect fifth is plainly audible I can never locate the beating solely in this tone. The primaries themselves always beat for

[6] *Phil. Studien*, XVII., p. 210.

[7] The mark ($^{-}$) indicates that the tone is slightly depressed, ($^{+}$) that it is slightly raised.

[8] *Phil. Stud.*, XVII., 1901.

me. Now when *three* generating tones are given in such ratio that their two *first* difference tones interfere, these primary tones are all heard to beat very plainly. Here are some intervals that I have found very good to illustrate this point:

$Ut_3:Mi_3:Sol_3^-$[9] ($4:5:6^-$, $5-4=1$ beats with $6^- - 5 = 1^-$),
$Ut_4:Mi_4:Sol_4^-$ ($4:5:6^-$),
$Ut_3^-:Sol_3:Ut_4$ ($2^-:3:4$), also the octave of these.
$Sol_3:Mi_4:7^-$[10] ($3:5:7$).

Several other intervals as good for this purpose might be added. In some cases a second difference tone may beat with a first, *e. g.*, $Ut_4:Mi_4:7^-$ ($4:5:7^-$). Here $2 \times 4 - 5 = 3$; $7^- - 4 = 3^-$.

Lord Kelvin,[11] indeed, pointed out this fact as early as 1878. He "found that the beats of an imperfectly tuned chord 3:4:5 were sometimes the very last sound heard, as the vibration of the forks died down, when the intensities of the three sounds chanced at the end to be suitably proportioned." When intervals of this kind are sounding loudly the *difference tones* are easily heard beating; but the primaries and the difference tones are so closely interconnected that the whole system of tones beats beautifully.

This last statement is less true when three primary tones are given in such relations that one of these tones beats with a difference or summation tone of the other two. Suppose, *e. g.*, that Ut_4 and Sol_4 are sounding loudly, and that while one hears these tones one holds Mi_5^- with a very feeble tone to one's ear. In such a case the tone Mi_5^- beats very plainly. In cases where the so-called auxiliary tone is very weak I simply hear a beating and no tone. The beating is then located distinctly at the pitch of the auxiliary. Only on very careful attention (and for me with the auxiliary fork sounding so loudly that it is audible as a tone) is beating also located in the other primary tones. The beating of the two lower primary tones, when heard, is always of the same frequency as that (louder beating) of the auxiliary

[9] The mark (⁻) indicates, as has been said, that the note is slightly flattened, by putting a piece of wax on one of the prongs. It is immaterial which note is flattened, except that high forks when thus weighted do not vibrate long.

[10] This is the 7th partial of Ut_1.

[11] *Proc. Roy. Soc. of Edin.*, IX., 1878, p. 602, cited by Rayleigh, *op. cit.*, p. 467.

tone. The forks may be interchanged and one of the lower ones used as the 'auxiliary' and, therefore, sounded more feebly and held to the ear. The results are much the same, *i. e.*, all three tones may still be heard to beat; but the highest tone, within certain limits, always, for me, beats most distinctly. Rapid beats especially are more easily heard with high tones than with low ones. This, of course, would be expected from Helmholtz's explanation of beats.

What was said in the last paragraph about locating beats also in the lower tones, applies especially to tones of such relations that the interval between the middle of three primaries and the upper tone (the auxiliary sounding feebly at the ear) is considerably smaller than an octave. The following intervals are illustrative, and were used in the experiment:

Interval	Auxiliary Fork
$Fa_3 : La_3$ (4 : 5)	Sol_5^- (9^-)
$Ut_4 : Mi_4$ (4 : 5)	Re_6^- [12] (9^-)
$Ut_3 : Sol_3$ (2 : 3)	Mi_5^- (5^-)
$Re_3b : La_3b$ (2 : 3)	Fa_5^- (5^-)
$Sol_3 : Ut_4$ (3 : 4)	T^- (T^-)
$Ut_3 : La_3$ (3 : 5)	Fa_5^- (8^-)
$Ut_3 : Mi_4$ (4 : 9)	T^- (13^-)

When one makes the auxiliary fork feeble enough in these cases and hears it only as a series of beats and not as a continuous tone, and, furthermore, when this seems in such cases to be the only beating, one can hardly avoid attributing the beats to the auxiliary tone with a summation tone which is present but not audible. The plausibility of this explanation is, of course, strengthened by the results of Krueger's experiment already referred to. His subjects actually heard the summation tones directly.

But against such an explanation of the beats of the auxiliary fork, it may be objected that these beats are not due to the presence of a summation tone at all but that they are explained easily from the fact illustrated above; viz., that generating tones are so closely associated with their difference tones, or resultants, that when the latter beat the former beat also. *E. g.*, in the first case of those given above it may be urged that a differ-

[12] We have not the Re_6 in the laboratory so I could not use the lower octave of this interval.

ence tone 4^- (from $9^- — 5$) interferes with the primary tone 4 and that the beats are located principally in the tone 9^-, the higher of the two primaries.

In addition to this objection another may be raised. It may be urged that, in general, beats cannot be located with any degree of certainty. This seems, to some extent, to be true from the results of Krueger's experiments. A very simple experiment may prove that the assertion made in this second objection has good grounds. Take, *e. g.,* two forks of like pitch, as two Ut_3 forks. Depress one slightly by the method explained above. Now, when these two forks are sounded together we have *one beating tone.* If *any second* tone is sounded simultaneously with this beating tone, the second tone is plainly heard to beat also. This second tone, as is true of the auxiliary above, may be sounded so feebly that only the *beats* are heard. The beats of this tone seem to alternate with those of the first in a fashion that may be represented in this way

Beating tone ——— ——— ———
etc.
'Second tone' ——— ——— ———,

This is very noticeable when the beats are slow. Whether this influence of a beating tone on other tones is or is not merely a psychological one[13] (and it probably is) is a question that has but little bearing on the present use of the phenomenon. The mere fact that there is such an effect produced by beating tones is one that must be reckoned with in all studies in which the localization of beats is important.

These objections make it doubtful whether the beating associated with the auxiliary forks above proves the existence of summation tones. Koenig's theory, as well as others that have been advanced, has no place for the first difference tones of large intervals approaching or exceeding the octave.[14] Now,

[13] It is well known, *e g.,* that a fluctuating intensity of light will subjectively affect other lights which are objectively constant. The above experiment, therefore, need not be regarded as affecting Helmholtz's physiological thory of tonal analysis.

[14] It is true that with auxiliary forks nearly attuned to the first difference tone, Koenig heard a beating for intervals as large as 8:15. He says nothing of where these beats *appeared* to be located. Cf. *Quelques Expérience d'Acoustique,* note, p. 130.

since the first objection of the two just considered, might be expected from Koenig, the following possibilities are open to us. We may take as generators two tones so near together in pitch that their summation tone, and hence the auxiliary used to detect it, lies nearly an octave above the higher of them. For this purpose I have used the following intervals:

Primary Tones	Auxiliary Tones
$Ut_3 : Re_3$ $256 + 288 = 544$	$Re_4\flat$ (17) of 546.1 vib.
$Re_3\sharp^- : Fa_3\sharp$ (nat.) $304.4^- + 341.7 = 646.1^-$	Mi_4 of 640 vib.
$Mi_3 : Fa_3\sharp$ $320 + 362 = 682$	Fa_4^- of 682.6^- vib.
$Fa_3\sharp : Sol_3\sharp$ (nat.) of $362 + 406.3 = 768.3$	Sol_4^- of 768^- vib.
$Fa_3\sharp : La_3\flat$ of $362 + 409.6 = 771.6$	Sol_4 of 768 vib.
$Fa_3\sharp : Sol_3\sharp$ of $362 + 400 = 762$	Sol_4^- of 768^- vib.
$La_3\flat : Si_3$ of $409.6 + 480 = 889.6$	7^- of 896^- vib.
$La_3\flat : Si_3$ (nat.) of $409.6^- + 483.2 = 892.8$	7^- of 896^- vib.
La_3 (nat.) : $La_3\sharp$ (nat.) $430.5 + 456.1 = 886.6$	7^- of 896^- vib.
$Si_3 : Re_4\flat$ of $480 + 546.1 = 1026.1$	Ut_5 of 1024 vib.
$La_3\sharp^-$ (nat.) : Ut_4 of $456.1^- + 512 = 968.1^-$	Si_4 of 960 vib.

In all these cases *the auxiliary fork was plainly heard to beat.* Many of the intervals, being dissonant, beat violently, but by varying the degree of depression of the auxiliary fork (or of one of the primaries as the case may be) its beating was easily discriminated from the other, more rapid, beating of the dissonant primary tones. Facts of this kind are not easily reconciled with a view like that of Koenig, *e. g.*, which denies the existence not only of summation tones but also of the first difference tone when the intervals are large. The summation tone, then, as indeed Krueger's results go to show, seems to be present with practically all intervals whether the vibration ratio is simple or not.[15]

Of course, *our* results are valid (if at all) only for very small intervals. Krueger, however, studied large intervals, as well as small ones.

Now, in the light of Krueger's results, it may be urged against the above evidence for summation tones, that intermediate difference tones and hence difference tones of large

[15] When this ratio is very complex an extremely high order of upper partial tones must be taken to give a difference tone equal in frequency to the summation tone of the primaries. Even the ratio 8 : 9 requires for this purpose, the presence of the 17th partials.

intervals do exist, whereas the above argument posits their non-existence. This objection is a valid one. But it is to be remarked that only those theories which already admit the existence of summation tones will urge this objection. With such theories we have no quarrel. All theories, however, which deny the existence of summation tones also deny that of difference tones of large intervals.[16]

The immediately foregoing statement is hardly necessary after the excellent work of Krueger. It is excusable, however, from the fact that Krueger studied summation tones only incidentally. But now we come to the important part of our investigation. *Are these supposed summation tones to be explained as difference tones of higher 'orders'?* This question Krueger has also settled, it seems to me, in case of all summation tones which are very weak. His generators were practically free from overtones, yet his observers frequently called attention to weak summation tones. But with certain intervals, *e. g.*, the fifth and the fourth, the summation tones are not hard to hear when the interval is not pitched high. Koenig admitted that the summation tones of these intervals had been heard directly in case of siren tones. Since these tones clearly possessed overtones he supposed that the summation tones generated by them were really 'beat-tones' of upper partials. Rücker and Edser showed that they are not such beat-tones. In later years, even such a close adherent to Helmholtz's view as K. L. Schaefer has supposed that summation tones which are 'audible under ordinary conditions' may be due to the presence of upper partials, *i. e.*, that in cases where they are perceptible with the unaided ear they are really difference tones.[17] Max Meyer, as we have seen, was of like mind. The results that follow refute such notions.

It will be recalled that two quite different possibilities have been suggested for explaining the summation tone as a mere difference tone:

[16] None of these statements can, of course, apply to Meyer's theory. He admits that on the Helmholtzian principle of transformation due to asymmetry in the drum these tones *may* originate in the middle ear. He rightly holds them to be very weak. On his view in this connection see above, pp. 91 ff.

[17] *Supra*, p. 101.

1. The first of these explanations, suggested by Apunn, Preyer, and others, makes the summation tone a difference tone of the 'second order' according to the formula

$$2h - (h - t) = h + t.$$

2. The other explanation, given by Koenig, makes the summation tone a difference tone of the 'first order' resulting from appropriate upper partials according to the equation

$$n(h - t) = h + t.$$

Schaefer's explanation, where this one requires too high orders of partial tones, may be represented thus $nh - mt = h + t$.[19]

The first of these views is closely allied to the one which derives the second difference tone from the first with one of the primaries, in this way:

$$t - (h - t) = 2t - h.$$

The intensity difficulty, as has often been pointed out, at once confronts this view. The following experiment is given simply to illustrate the difficulty. The two forks Ut_4 and Mi_4 were fastened securely to the side of the table in such a position that their resonance boxes pointed in a vertical direction. They were then simultaneously actuated by dropping on their horizontally projecting prongs two small rubber stoppers each weighing 13.2 gr. The stoppers fell on the prongs near the end and immediately bounded off. To prevent any noise a piece of twine was attached to each stopper and held so that the stopper could not fall on the floor. By this means it was possible to regulate fairly well the intensities of the primaries for purposes of comparison. Both of the forks used gave very clear tones without any perceptible overtones for the intensities used. Mi_4 continued sounding longer, however, when the two were actuated with about the same force.

Now it was found, on various tests of this kind, that the *second* difference tone, Sol_3 was easily perceptible when the stoppers dropped upon the prongs of the forks from a position 4.7 cm. above them. The primary tones in such cases were

[19] n and m stand for whole numbers, h and t, of course, for upper and lower primary tones respectively.

themselves very feeble. The second difference tone, it was found by the use of a stop watch, continued to be audible for from 5 to 16 seconds after the forks were actuated. There was yet absolutely no trace of the first difference tone Ut_2. The distance through which the stoppers had to fall was now increased gradually until this tone became audible, too. This occurred when the falling distance became 50 to 56 cm. These figures were confirmed by tests in which the drop-distances of the weights were gradually decreased from a position at which both difference tones were audible to positions where they became inaudible; first the *first*, then the *second* difference tone disappeared. While the second difference tone was smooth and clear and continued relatively long [nearly as long as the lower primary], the lower first difference tone was rough and soon disappeared. With suitable forks, such, *e. g.*, as the new Edelmann set, this experiment could profitably be carried out to considerable length. Various intervals might be tested in different parts of the scale. The above facts, however, seem conclusive against the view that the second difference tone always arises from the first difference tone and the higher primary tone. This view, therefore, cannot be used to refute by analogy as suggested the existence of real summation tones in the Helmholtzian sense.

It is this same interval that Hermann and others used as illustrative of the intensity relations which they considered contradictory to Helmholtz's theory of different 'orders' of combination tones. The objection has less force when urged against his mathematical theory. According to this theory, as has been suggested, all the combination tones arise *directly* from the primaries. The intensity relations for this same interval (the major third) differ with different pitches of the primaries. *E. g.*, when Ut_3 and Mi_3 are used as the primary tones, the first difference tone appears, for me, before the second as the intensity of the primaries increases. When the primaries are Ut_5 and Mi_5 the two difference tones usually appear and disappear together, lasting but a very short time. In this latter case the primaries themselves soon 'run out.' For

different reasons I could not apply so accurate quantitative measurement to these cases as to the one first described.[19]

If the difference tone $2t - h$ cannot be explained in the manner suggested by the equation

$$t - (h - t) = 2t - h,$$

it is even more improbable that Preyer's explanation of the summation tone, *i. e.*, $2h - (h - t) = h + t$, will hold. Here we not only have an upper partial serving as a primary tone, but we have to do with a very large interval as well, *i. e.*, $(h - t) : 2h$. But the explanation is not hard to test.

The summation tone of the interval $Ut_3 : Sol_3$ was found unmistakably to be audible to the unaided ear.[20] The tone of the fork Mi_4^- beats with it very plainly (we disregard here the possibility of other interpretations of these beats). Preyer would explain this summation tone as follows: $Sol_4 - Ut_2 = Mi_4$ [*i. e.*, $2 \times 384 - (384 - 256) = 640$]. Now I sounded very loudly the forks Sol_4 and Ut_2, but heard no difference tone at all. Mi_4^- was then used as an auxiliary tone. The primaries were again sounded loudly while the fork Mi_4^- sounding very feebly was held to the ear. Several tests were made with different degrees of depression of the auxiliary tone. Only *very* faint beats were noticeable,[21] showing that an exceedingly weak difference tone was produced. To be sure that beats were actually heard Mi_4^- was afterward sounded with another Mi_4 fork. The beats had the same frequency. The experiment was repeated with the corresponding tones of the next octave above, *i. e.*, Sol_5 and Ut_3, Mi_5^- being used as the auxiliary. Similar results were obtained. It is to be noted that the summation tone of $Ut_4 : Sol_4$ was not directly audible to those of us who heard it with the interval an octave lower.[22]

The only other test of the kind possible with the forks available was one with the fourth, $Sol_3 : Ut_4$. The sum of

[19] We are very much in need of a careful study of intensities with generators that can be relied upon.

[20] See below, p. 125.

[21] And these *may* have been due to a low difference tone Ut_1' (from $Sol_4 - Mi_4^-$) interfering with Ut_1.

[22] I seemed to hear it occasionally after special practice but was always uncertain; the low difference tone was very loud and rough.

the vibrations of these forks is 896, *i. e.*, the vibration number of the fork 7. The summation tone in this case was inaudible, but beat very plainly with the weak tone of 896^- vibrations. The same result was obtained in this case as in those above, viz., the difference tones of the forks $Ut_2 : Ut_5$ (1 : 8) beat *very* feebly with the auxiliary tone.

In all three of the cases just considered, the forks taken to represent the interval $(h - t) : 2h$ were sounded with considerable intensity. When they give more feeble tones, but certainly louder than the difference tone and the first upper partial for which they are substituted, *purely negative results were obtained:* the auxiliary tone revealed no beats whatever. These few experiments seem conclusive against such an account of summation tones as that given by Preyer and others. The explanation, it is true, is intended to account only for summation tones which are actually audible. We may attach considerable importance to the *one* such case here examined. The result of that one case seems absolutely decisive against the explanation in question.

We shall now consider the second type of view—*i. e.,* that of Koenig—which regards the summation tone as a first difference tone (beat-tone) of upper partials of the same order. Is such an explanation probable, or even possible, in *any* case?

In the first place tests with auxiliary forks for the existence of upper partials contradict the view. The second partial, *i. e.,* the first upper partial, of Ut_3 was audible to the unaided ear. The auxiliary fork beat with partials as high as the fifth inclusive. When the fork Ut_3 was damped by holding one prong near the stem with thumb and a finger, the fifth partial seemed to drop out, leaving only four present. Of Sol_3 the second partial was audible. The auxiliary tone beat with partials as high as the fourth. No fork was available to test for the fifth, but the fourth was so weak that the fifth probably did not exist. Damping does not seem to affect the upper partials as much as it was at first supposed. I am not sure that the fourth partial in this case was eliminated by damping. Of Mi_3 the partials as high as the fourth beat plainly with the auxiliary tone. I did not extend this test to the other forks

used below, but it may be safe from the above results to say that if partials beyond the fifth are present at all, they are extremely weak.[23]

If upper partial tones are effective in the production of combination tones, as some persons have supposed, this fact should easily be discoverable in the case of the second difference tone. As has already been stated the second difference tone (Sol_3) of the interval $Ut_4:Mi_4$ is very prominent even when the primaries are sounded very feebly. This difference tone is clear and continues nearly as long as the lower of the primary tones.[24] When the primaries are made stronger the *first* difference tone appears. It is rough and lasts but a short time. *Now, when Ut_5 is substituted for Ut_4 in this interval, the difference tone Sol_3 should, as a rule, be even more prominent, if it is dependent upon the upper partial of the lower tone of the interval $Ut_4:Mi_4$,* as has been supposed from the pitch $2t - h$. But when the substitution is made, this difference tone at once loses its clearness and takes on the characteristics of the *first* difference tone just described. Moreover, the primaries must now be sounded considerably louder for the appearance of this tone. This seems to show conclusively that the *upper partial of Ut_4 in the interval $Ut_4:Mi_4$ has practically nothing at all to do with the generation of the second difference tone Sol_3.* Meyer and Krueger by another method have indeed already proved more or less conclusively that combination tones are practically not at all dependent upon upper partials of the primaries, so further tests along the line indicated here would be needless even if the necessary forks were at hand.

What holds for difference tones with respect to upper partials ought also to hold for summation tones—especially if the latter *are* difference tones! But since Koenig and others, and even as recent an experimenter as K. L. Schaefer, have attributed audible summation tones to upper partials, it seems necessary to investigate the matter further.

[23] It is, of course, true that occasionally a very high loud partial is heard, but such cases are usually irregular and the partial may readily be damped out without affecting the combination tones. The experiment following, anyway, rules out the possibility of the effectiveness, for the production of summation tones, of upper partials.

[24] Mi_4, it will be remembered, continues longer than Ut_4.

If the fifth partials of the fifth (2:3) produce an audible difference tone ($5 \times 3 - 5 \times 2 = 5$) it is reasonable to suppose that in most cases partials below these produce even louder difference tones.[25] The second partials should give rise to the tone 2 (*i. e.*, $2 \times 3 - 2 \times 2$) the third to the tone 3; the fourth to 4. These tones all correspond to the primaries or to some of their upper partials. The case is different with the major third (4:5). If we look at the upper partials

4, 8, 12, 16, 20, 24, 28, 32, 36, . . .
5, 10, 15, 20, 25, 30, 35, 40, 45, . . .

it is evident that besides the tone 9 (*i. e.*, $45 - 36$) we ought to hear (other than upper partials and primaries) tones corresponding to 1, 2, 3, 6 and 7, according to Koenig's explanation of summation tones. The tones 1 and 3 are also difference tones of the primaries, however. We can test, then, only for 2, 6 and 7.

When the forks Ut_3 and Mi_3 are sounded loudly together their first upper partials are audible but very rough, due evidently to the summation tone lying between them.[26] This is much less noticeable when the interval is pitched an octave higher, but a very weak auxiliary tone of pitch nearly equal to that of the summation tone beats plainly. Now is there any evidence of the existence of those other tones which we ought to hear if Koenig is right?

Using the primaries $Ut_3 : Mi_3$ (4:5) we may test with the auxiliary fork Sol_3^- for the tone corresponding to 6. When the primaries are sounding very loudly and Sol_3^-, sounding very feebly, is held to the ear the tone is actually heard to beat plainly. In this case, however, the beating is no proof of the existence of the tone 6. It is doubtless due here to the interference of two first difference tones as is evident from these numbers:

$$6^- - 5 = 1^- \text{ and } 5 - 4 = 1.$$

It will be recalled that in cases of this kind the beating is very

[25] See the note just preceding this, however.

[26] Cf. Krueger's remarks on the intertone (Zwischenton) in *Phil. Studien*, XVII., 1901.

marked. When the auxiliary tone in this case is sounded more strongly these difference tones are actually heard beating.

In order to test for the tones 2 and 7 we take the primaries an octave higher (Ut_4:Mi_4) and use the auxiliary forks Ut_3^- and 7^- (896^-). Ut_3^- beats slightly but unmistakably with the primaries, but the frequency of these beats is twice that of those which Ut_3^- makes with the other fork Ut_3. The rapid feeble beats heard are due then to the interference of the upper partial of Ut_3^- with the primary tone Ut_4. This is clearly evident when the intensity of the auxiliary fork is increased. When, however, the auxiliary fork is sounding so feebly that upper partials do not occur, *the beats entirely disappear.* The tone 2, therefore, very probably does not exist. On the other hand the auxiliary fork 7^- beats very plainly with the primaries even though it is very weak. This beating, however, is not due to another tone corresponding to 7, but is the beating due to difference tones as shown by the equations

$$7^- - 4 = 3^-$$

and

$$2 \times 4 - 5 = 3.$$

This is easily proved in this way: Instead of depressing the auxiliary fork 7, make Ut_4 (4) flat enough to beat twice in a second, *e. g.*, with another Ut_4 fork. Now the auxiliary fork beats six times a second with the primaries. This is because when the tone Ut_4 (represented by 4 in the equations just given) is depressed two beats, its second partial (2×4) is depressed four beats. An illustration with actual vibration numbers may make this clearer. The beating of the auxiliary fork, 7 (896) depressed to say 894 vibrations per second with the primaries Ut_4:Mi_4 (512:640) is due to the interference of the two difference tones.

$$382 \text{ (from } 894 - 512, \textit{ i. e., } \dot{7} - Ut_4)$$

and

$$384 \text{ (from } 2 \times 512 - 640, \textit{ i. e., } 2 \times Ut_4 - Mi_4).$$

If instead of depressing 7 we depress Ut_4 two beats per second, *i. e.*, to 510 vibrations, the beating of the auxiliary tone 7, held

to the ear, has a frequency of *six* per second as is evident from the equations:

$$896 - 510 = 386$$
$$2 \times 510 - 640 = 380.$$

When, however, the tone Mi_4 (640) instead of Ut_4 is depressed two beats per second the auxiliary fork beats only twice per second, as the explanation just given evidently requires. The beating of the auxiliary tones, therefore, which was heard in testing for the tones 2 and 7, corresponding to beat-tones of upper partials of the major third (4:5) does not prove the existence of these alleged 'beat-tones,' but *must* be explained otherwise. That such 'beat-tones,' arising from partials lower than those which give rise (according to Koenig) to the summation tone, do not exist is proved, then, (1) by the fact that in several cases tested the auxiliary tone, intended to interfere with them, does not beat at all; (2) by the fact that whenever the auxiliary tone does beat, the beating is attributable to low difference tones, *and does not have the frequency required by Koenig's supposition.* In the illustration above, where the beating of the auxiliary fork had a frequency of *six* per second when Ut_4 was depressed *two* vibrations, it is evident that if the tone 7 existed at all and was produced by a 'beat-tone' of the seventh partials of the primaries, *the frequency of the beats should have been seven times two per second, since the seventh partials would be depressed seven times as many beats as the first.* On this point we shall dwell more in detail very soon.

We have assumed, then, on very good ground, that if the summation tone be a 'beat-tone' of upper partials, as Koenig suggested, several other 'beat-tones' of lower partials should be even more plainly audible; and it has now been proved conclusively, I think, that such other 'beat-tones' *do not exist.* Aside from the proof here given it should be noted that *these lower 'beat-tones' of upper partials are never audible*[27] *to the unaided ear,* whereas, the summation tone frequently is audible.

[27] At any rate I have never been able to hear them and no one else seems to have done so. Hällström thought he heard a tone $2h - 2t$, as has been seen, but was probably deceived. Of course, this tone may exist when the second

We have yet one more method of attack which meets even more directly the theories here opposed, and which must hold quite independently of the validity of the use of auxiliary tones in establishing the *existence* of summation tones. The principle of this method has already been suggested.

After having convinced myself that the summation tones of the primaries $Ut_3:Sol_3$ (2:3), $Re_3♭:La_3♭$ (2:3), $Fa_3:La_3$ (4:5) and $Ut_3:La_3$ (3:5) are audible to the unaided ear, I experimented with other students of the laboratory to assure myself that the summation tones were not imaginary. The students selected were all trained in experimental psychology. Those who served in this connection were Miss Elizabeth R. Shaw, Miss Florence E. Richardson, and Miss Grace M. Fernald. Each subject was asked to select from a number of high pitched forks the tones that were audible in addition to the primaries, when certain intervals were sounded. Only one of the subjects was present at a time, and in no case did she know which fork of the group represented the summation tone. The primaries were sounded a little above medium intensity with a rubber hammer. The intervals used were those just named. At least one of the second partials of the primaries was always heard. The fork representing the summation tone was in every case selected. Some of the subjects found it more easily than others. No tone, other than the summation tone or the first upper partial tones, was ever selected as a final judgment, even though in some cases the experimenter called special attention to other tones to try the force of suggestion. In some cases the subject readily sang the note representing the summation tone. It seems evident from these tests that the summation tones of these intervals are actually audible.

Now these summation tones ought to beat with auxiliary tones of nearly their pitch, and on this basis it should be possible to ascertain some facts concerning the mode of origin of these tones. If the summation tone is a difference tone, or a 'beat-tone,' of upper partials this should be revealed by the frequency

partials are strong, but its existence will not affect the above argument. In any case it ought to be very much stronger than the summation tone if Koenig's view be correct.

of its beats with an auxiliary tone. From these numbers, representing the fifth with its upper partials,

$$\begin{array}{ccccc} 2, & 4, & 6, & 8, & 10, \ldots \\ 3, & 6, & 9, & 12, & 15, \ldots \end{array}$$

it is evident that if the tone represented by 2 is depressed one vibration, the second partial, 4, will be depressed *two* vibrations; the third, 6, three vibrations; the fourth, 8, four vibrations; the fifth, 10, five vibrations; and so on. This is easily proved thus: if Ut_3^- beats once per second with Ut_3 it is found to beat twice with Ut_4, three times with Sol_4, and so on. Now if Koenig's explanation of summation tones as 'beat-tones' of appropriate upper partials, be correct, it is evident that *a depression of one vibration of the primaries of the fifth will depress the summation tone five vibrations. From this fact we can make a decisive test of the validity of this explanation.*

In the attempt to test this explanation I soon encountered a difficulty, one, however, which was not insuperable. When one of the primaries is depressed the interval becomes imperfect and the dissonant beats interfere with a careful study of the summation tone, but the beats due to the imperfection of the interval are distinguishable from those of the summation tone with the auxiliary of nearly the same pitch. To get rid of the beats of the imperfect interval the following procedure was adopted. First, after one of the primary tones had been depressed one vibration per second, the auxiliary fork was gradually depressed until the only beats remaining were those of the imperfect interval. In the case of the fifth given above these beats had a frequency of three per second when the *lower* primary tone was depressed one vibration.

This is evident from the following numbers:

384 is the vibration number of the higher tone.

255 is the vibration number of the depressed lower primary tone.

129 is the first difference tone.

126 (*i. e.*, $255 \times 2 - 384$) is the second difference tone.

3 is the number of beats per second due to the interference of these difference tones.

After this adjustment of the auxiliary tone the lower primary tone was again raised to its true pitch. The only beats then remaining were those resulting from the interference of the auxiliary with the summation tone, and possibly also beats of the inaudible difference tone (produced by the auxiliary tone with the upper primary) interfering with the lower primary thus:

$$5^{-} - 3 = 2^{-} \text{ which beats with } 2.$$

Now it is evident that raising the depressed primary tone one vibration to its true pitch should make the difference-tone-beating have a frequency of *one* per second (since the difference does not depend upon any upper partial), while the summation tone, if Koenig's explanation of it be true, should be raised five vibrations and hence should beat *five* times per second with the auxiliary tone. One ought, therefore to hear two sets of beats: (*a*) a slow beating with a frequency of one per second, due to the difference tone, if it is present; (*b*) a rapid beating with a frequency of five per second due to the summation tone. *There is absolutely no trace of such rapid beats, whereas the slow beats are very perceptible and can easily be counted.* The intervals tested in this way are Ut_3:Sol_3 (2:3), $Re_3\flat$:$La_3\flat$ (2:3), Fa_3:La_3 (4:5), and Ut_3:La_3 (3:5). In every case the summation tone was audible to the unaided ear and beyond question should beat with the auxiliary tone in tests such as the one just described. The conclusion seems inevitable, that the slow (the only) beating heard is due, at least in part, probably entirely, to the summation tone, and that consequently *the summation tone is depressed the same number of vibrations as the primary tone is depressed,* in accordance with Helmholtz's view.

These experiments, disproving the valadity of Koenig's explanation of summation tones, apply with equal force to explanations like that of K. L. Schaefer, illustrated by the equation

$$mh - nt = h + t.$$

We are, therefore, justified in the conclusion that the second class of views, regarding the summation tone as a first difference tone of appropriate upper partials is also wrong.

Our conclusion which is in harmony with Helmholtz's theory of combination tones, agrees entirely with the results of Krueger's recent experiments in which he found that subjective summation tones are audible even when upper partials of the primaries are eliminated. On the whole, *experimental evidence contradicts any theory which regards summation tones as dependent upon upper partial tones or difference tones of the primaries.*

Summary of Data Bearing upon the Ohm's Law Question.

Objections to the Ohm-Helmholtzian View.	*Replies and Points Favoring the view.*
1. Upper partial tones actually present are heard either relatively weak or not at all (Seebeck).	1. Until occasion is afforded to analyze the tones of a clang we perceive *synthetically*. By proper training we can perceive the upper partials relatively louder than usual (Helmholtz).
2. The vibration numbers of difference tones equal the frequency of the beats of the primaries (Koenig, *et al.*).	2. No beats thus correspond to the summation tone (Helmholtz).
3. Large intervals may beat even when there are no overtones present (Koenig, Wundt, *et al.*).	3. Such beats, when there are no overtones to produce them, are due to the presence of difference tones which may be too weak to be directly heard (Helmholtz, Bosanquet, Krueger).
4. 'Interruption tones' are heard when a given tone is periodically intermitted or partially so intermitted (Koenig, Dennert, *et al.*).	4. These tones can be resonated with physical resonators, proving that they correspond to physical pendular vibrations (Schaefer and Abraham).
5. Phase changing vibrations are experienced by the ear as tones (Koenig, Hermann, *et al.*). The ear detects phase differences and	5. These *supposed* phase changing vibrations have been resonated with physical resonators, proving that they are actually sinus-form

employs them in localization of sound (Rayleigh).	(Schaefer and Abraham).
6. Shifts of phase in upper partial tones of a clang affect the quality or timbre of the clang (Koenig). The change in timbre is due not to shift of phase but to change in objective form of the *compound wave* (Hermann).	6. It is probable that the instrument used (the wave-siren) did not reproduce with accuracy these shifts of phase (Hermann); anyway the change in timbre is but slight. The second statement has never been proved.
7. The intensities of combination tones are not experienced in the proportions required according to the Helmholtzian theory (Voigt, Stumpf, Hermann, Meyer, *et al.*).	7. (Not satisfactorily answered by any theory.)
8. A low tone may obliterate a higher one but the reverse is not true (A. M. Mayer).	8.
9. Two tones conducted separately one to each ear can be heard to beat (Cross and Goodwin).	9. We cannot be quite sure that by means of bone conduction, or by some other mode of conduction, both tones did not operate in the same ear.
10. (The statement opposite this space is of course not necessarily contradictory to the opposition against Ohm's law.)	10. Helmholtz's theory of objective combination tones is supported by the fact that such tones have been resonated with various physical resonators (Helmholtz, Rücker and Edser, Forsyth and Sowter, Schaefer).
11. The intertone (Zwischenton) is contradictory to a strict interpretation of Ohm's law.	11. It is explained without serious difficulty on the basis of 'physiological adaptation of nerves' (Stumpf). This tone is not so clear and distinct as might be supposed from some accounts; it is more nearly a 'beating complex' than a tone (Krueger, Peterson).
12. Only such summation tones exist as are attributable (as difference tones) to upper partials	12. Very weak summation tones can in most cases be heard on sufficient practice, or training of

either wholly (Koenig) or in part (Appunn, Preyer, *et al.*).

the ear, and attention (Krueger); 'objective' summation tones are not explicable as difference tones (Rücker and Edser); 'subjective' summation tones are not explicable as difference tones (Peterson).

13. Both 'objective' (Rücker and Edser) and 'subjective' (Krueger) intermediate (zwischenliegenden) difference tones exist.

TABULAR STATEMENT OF THE EFFICIENCY OF THEORIES DISCUSSED IN THIS THESIS.

Fact to be Explained.	Ohm-Helmholtz.	Young-Koenig.	Hermann.	Helmholtz-Wundt.	Helmholtz-Ebbinghaus.	Meyer.
Analysis of complex clangs....................	×	?	?	×	×	×
The common phenomenon of beats............	×	×	×	×	×	×
Beats of large intervals of pure tones..........	×	×	?	×	×(?)	×
Common difference tones......................	×	×	?	×	×(?)	×
Objective combination tones..................	×	?	?	×	×(?)	×(?)
Intermediate first difference tone (subjective)...	×			?	?	?
Upper and lower 'beat-tones' (?).............	×(?)	×	×	?	?	×(?)
Summation tones.............................	×			?		?
Intensity relations of combination tones.........	?	?	?	?	?	?
A lower may obliterate a higher tone...........						×
The ear experiences phase differences (?).......	?	×	×	?	?	×
Two tones heard simultaneously, one by each ear (?), beat.............................	?	?	?	×	?	?
The phenomenon of the intertone (Zwischenton)	×(?)		?	?		?
"Tone islands," *Diplacusis binauralis dysharmonica* (pathological phenomena not mentioned in this thesis)............................	×		?			

A cross (×) means that the theory can explain the fact opposite which it occurs.

A question mark (?) signifies either that the efficiency of the theory is doubtful or that the theory needs modification or supplement.

Note the inconsistencies in Wundt's and Ebbinghaus's theories pointed out in the text.

APPENDIX A.

An experiment reported recently by Exner and Pollak[1] is somewhat closely allied to that of Mayer, and may be mentioned here. I quote from the succinct statement of Bentley and Sabine:

"They purpose to test the resonator theory of audition by using simple tones with periodic reversal of phase. They reason as follows. When a wave train acts on a properly tuned resonator, the effect up to a certain limit is cumulative; *i. e.*, each successive wave increases the sympathetic vibration of the resonator until the limit is reached. If, however, the wave suddenly changes phase, its energy will be directed against the inertia of the resonator, and the two will oppose one another until equilibrium is reached, after which the wave will again produce on the resonator its former cumulative effect. If, now, this change of phase is made periodically, it should result in a wave with much smaller amplitude than the original wave, periodically varying in intensity, unless the phase changes follow one another so closely that the wave is entirely annihilated. Hence it should follow that, if audition is mediated by a series of resonators, a tone thus interrupted should be discontinuous and we should hear bursts of sound alternating with periods of silence. It should further follow that, by keeping the intensity of the tone constant and increasing the frequency of the phase changes, we can cause the tone to decrease in intensity until it entirely disappears. That is, the cumulative effect on the basilar resonators of the waves following between any two successive phase changes will not be sufficient to raise the nervous impulse above the limen of sensibility. If, now, the number of phase changes is kept constant and the physical intensity of the tone is increased, the tone which has become just inaudible should be lifted over the limen.

"Exner and Pollak used three forms of experiment to obtain the conditions which they required: (1) a tuning fork rotated about the longitudinal axis of its stem and having, there-

[1] Sigm. Exner u. Jos. Pollak, 'Beitrag zur Resonanztheorie der Tonempfindungen,' *Zeitschr. f. Psychol.*, XXXII., 1903, pp. 305–332.

fore, four phase changes for each revolution; (2) a stationary fork which actuated a telephonic diaphragm under a current which was periodically reversed by means of a rotating commutator, thus causing two changes of phase at each revolution; (3) a rotating stopcock which brought alternately to the ear the waves from the side and from the face of a continuously sounding fork. The results reached by these methods confirm the authors' hypothesis regarding auditory resonance. They found that the sudden reversal of phase, when it comes with sufficient frequency, destroys the tone. A critical rate of phase change was discovered [*e. g.*, 10 vibrations between each two successive phase changes for a tone of 240 vibration]. At this rate (which was fairly constant under the given conditions) the sound of the tuning fork disappeared, and reappeared only when the rate of revolution was diminished."[2]

Bentley and Sabine repeated the experiment under somewhat similar conditions.[3]

Their results did not agree entirely with those given above. They promise another article which, I believe, has not yet appeared.

Dr. Geo. E. Shambaugh has recently made a very careful study of the tectorial membrane in the pig's ear.[4] He found that this organ, whatever its function, increases enormously in size from the base of the cochlea toward the apex, and suggests that this structure may serve as the resonant analytic organ in hearing. It is doubtful, however, whether this organ, despite its fibrillar structure, can act as a resonator. No physical resonators of this kind are known, or described by physicists. Dr. Shambaugh finds that at the base of the cochlea the basilar membrane is completely underlaid by a bony bridge.

K. Kishi,[5] on the other hand, finds that the basilar membrane is not so unfavorable, in its structure, to a view like that of Helmholtz.

[2] *Am. Jour. of Psychol.*, XVI., 1904, pp. 489–90.

[3] 'A Study of Tone Analysis,' *ibid.*, pp. 484–98.

[4] George E. Shambaugh, A Restudy of the Minute Anatomy of Structures in the Cochlea with Conclusions bearing on the Problem of Tone Perception, *Amer. Journal of Anatomy*, 1907, VII., 246–257.

[5] K. Kishi, 'Cortische Membran und Tonempfindungentheorie,' *Pfluger's Archiv*, CXVI., 1907, 112 ff.

BIBLIOGRAPHY.

Works Quoted or Referred to in the Text.

A. Sorge. Vorgemach der musikal. Composition, 1745–47.

De la Grange. Nouvelles recherches sur la nature et la propogation du son. *Miscel. phil.-math. soc. priv.,* Taurinensis, T. I., 1759.

Chladni. Akustik, 1802.

E. H. and W. Weber. Wellenlehre, 1825.

Le baron Blein. Exposé de quelques expériences nouveaux sur l'acoustique, etc., 1827.

W. Weber. Ueber die Tartini'schen Töne. *Pogg. Annal. d. Physik,* XV., 1829.

G. G. Hällström. Von den Combinationstönen. *Pogg. Annal.,* XXIV., 1832.

A. Roeber. Untersuchung des Herrn Scheibler, etc. *Pogg. Annal.,* XXXII., 1834.

G. S. Ohm. Articles on the definition of a tone. *Pogg. Annal.,* LIX., 1843; and LXII., 1844.

A. Siebeck. Articles in *Pogg. Annal.,* LX., 1843; LXIII., 1844.

Peacock. Works of Dr. Young, Vol. I., 1855.

H. Helmholtz. Ueber Combinationstöne. *Pogg. Annal.,* XCIX., 1856.

Die Lehre von Tonempfindungen, 1863.

Sensations of Tone, 3d ed. of Ellis' translation, 1895.

E. Mach. Ueber die Aenderung des Tones und der Farbe durch Bewegung. *Pogg. Annal.,* CXII., 1861.

Radau. Moniteur Scientifique, 1865.

J. Stefan. Ueber einen akustischen Versuch. *Sitzungsber. d. kais. Akad. d. Wiss. zu Wien, Math.-Naturwiss.,* Kl. LIII., abt. 2, and Kl. LIV., abt. 2, 1866.

W. Beetz. Ueber die Töne rotierender Stimmgabeln. *Pogg. Annal.,* CXXVIII., 1866 (trans. by G. C. Foster, *Phil. Mag.,* 4th Series, XXXII., 1866).

A second article, same title, *Pogg. Annal.,* CXXX., 1867.

W. Beetz. Ueber den Einfluss der Bewegung der Tonquelle auf die Tonhöhe. *Pogg. Annal.,* CXXX., 1867.

A. M. Mayer. Researches in Acoustics. *Phil. Mag.,* 4th Series, XLIX., 1875.

A Redetermination of the Constants of the Law Connecting the Pitch of a Sound with the duration of its Residual Sensation. *Am. Jour. of Science and Arts,* CIX., 1873.

Researches in Acoustics. *Phil. Mag.,* 5th Series, II., 1876.

On the Production of Beat-tones from two Vibrating Bodies whose Frequencies are so High as to be Separately Inaudible. *Rep. of the Brit. Assoc. for the Adv. of Science,* 1894.

SIR WM. THOMSON. Beats of Imperfect Harmonics. *Proc. of the Roy. Soc. Edin.,* IX., 1876.

R. KOENIG. Ueber den Zusammenklang zweier Töne. *Pogg. Annal.,* CLVII., 1876.

Bemerkungen über die Klangfarbe. *Wied. Annal.,* XIV., 1881.

Ueber den Ursprung der Stösse u. Stössetöne. *Wied. Annal.,* XI., 1881.

Quelques Expériences d'Acoustique, 1881.

W. PREYER. Akustiche Untersuchungen, II., 1879.

Ueber Combinationstöne. *Wied. Annal.,* XXXVIII., 1889.

R. H. M. BOSANQUET. On the Beats of Consonances of the form $h:1$. *Phil. Mag.,* 5th Series XI., 1881.

O. LUMMER. Ueber eine empfindl. obj. Klanganalyse. *Verhandl. d. Berl. phys. Gesellsch.,* 1886.

H. DENNERT. Akustisch.-physiol. Untersuchungen. *Archiv f. Ohrenheilkunde,* XXIV., 1887.

MAX WIEN. Ueber d. Messung der Tonstärke. *Wied Annal.,* XXXVI., 1889.

W. VOIGT. Ueber d. Zusammenklang zweier einfache Töne. *Wied. Annal.,* XL., 1890.

C. STUMPF. Tonpsychologie, II., 1890.

Ueber die Ermittelung von Obertöne. *Wied. Annal.,* LVII., 1896.

Ueber zusammengesetzte Wellenformen. *Zeitschr. f. Psych.,* XXXIX., 1905.

CROSS and GOODWIN. Some Considerations Regarding Helmholtz's Theory of Consonance. *Proc. Am. Acad. of Arts and Sciences,* XXVII., 1891.

L. HERMANN. Phonophotographische Untersuchungen. *Pflüger's Archiv,* XLVII., 1890.

Zur Theorie der Comb.-töne. *Ibid.,* XLIX., 1891.

Phonophotographische Untersuchungen. *Ibid.,* LIII., 1892.

Beitrage Zur Lehre von der Klangwahrnehmung. *Ibid.,* LVI., 1894.

J. A. Zahm. Sound and Music, 1892.

R. Ewald. Physiol. Unters. über des Endorgan des Nervus Opticus. *Wiesbaden,* 1892.

W. Wundt. Ist der Hörnerv durch Tonschwingungen erregbar? *Phil. Studien,* VIII., 1893.

Akustische Versuche an einer labyrinthlosen Taube. *Ibid.,* IX., 1894.

Physiologische Psychologie, 5th ed., II., 1902.

H. Pipping. Zur Lehre v. d. Vocalklängen. *Zeitschr. f. Biologie,* XXXI. (N. F. XIII.), 1895.

Rücker and Edser. Obj. Reality of Comb. Tones. *Phil. Mag.,* 5th Series, XXXIX., 1895.

Lord Rayleigh. Theory of Sound, revised, Vol. I., 1894; Vol. II., 1896.

Acoustical Observations. *Phil. Mag.,* 5th Series, IX., 1880.

On Our Perception of Sound Direction. *Phil. Mag.,* XIII., No. 74, 1907.

Acoustical Notes. *Ibid.,* No. 75, 1907.

Max Meyer. Uber Kombinationstöne, etc. *Zeitschr. f. Psychologie,* XI., 1896.

Zur Theorie der Differenztöne u. der Gehörsempfindungen überhaupt. *Ibid.,* XVI., 1898.

Ueber die Intensität der Einzeltöne zusammengesetzter Klänge. *Ibid.,* XVII, 1898.

Karl L. Schaefer's "Neue Erklärung der Sub. Comb.-töne." *Pflüger's Archiv,* LXXXI., 1900.

Über Kombinations- und Asymmetrietöne. *Drude's Annal.,* XII., 1903.

Auditory Sensations in an El. Lab. Course. *Am. Jour. of Psych.,* XVI., 1905.

Forsyth and Sowter. On Photographic Evidence of the Objective Reality of Combination Tones. *Proc. of the Royal Soc. of London,* LXIII., 1898.

K. L. Schaefer. Eine neue Erklärung der sub. Combinationstöne auf Grund d. Helmholtz'schen Resonanzhypothese. *Pflüger's Archiv,* LXXVIII., 1899.

Weitere Bemerkungen zu meine "Neue Erklärung," etc. *Ibid.,* LXXXIII., 1900.

Über die Erzeugung physikalischer Kombinationstöne mittels des Stentortelephons. *Drude's Annal.,* XVII., 1905.

K. L. Schaefer und O. Abraham. Studien über Unterbrechungstöne.

Pflüger's Archiv, LXXXIII., 1901; LXXXV., 1901; and LXXXVIII., 1902.

Zur Lehre von den sog. Unterbrechungstönen. *Drude's Annal.,* XIII., 1904.

E. A. SCHAEFER. A Text-Book of Physiology, Vol. II., 1900.

H. ZWAARDEMAKER. Ueber Intermittenztöne. Engelmann's *Archiv f. Physiologie,* Sup. Bd., 1900.

F. KRUEGER. Beobachtungen an Zweiklängen. *Phil. Studien,* XVI., 1900.

Zur Theorie der Combinationstöne. *Ibid.,* XVII., 1901.

H. EBBINGHAUS. Grundzüge der Psychologie, I., 1902. Rev. ed., 1905.

EXNER und POLLAK. Beitrag zur Resonanztheorie der Tonempfindungen. *Zeitschr. f. Psychologie,* XXXII., 1903.

BENTLEY and TITCHENER. Ebbinghaus' Explanation of Beats. *Am. Jour. of Psych.,* XV., 1904.

BENTLEY and SABINE. A Study of Tonal Analysis, I. *Am. Jour. of Psychol.,* XVI., 1905.

NAGEL. Physiologie des Menschen, III., 1905.

K. KISHI. Cortische Membran und Tonempfindungentheorie. *Pflüger's Archiv,* CXVI., 1907.

GEORGE E. SHAMBAUGH. A Restudy of the Minute Anatomy of Structures in the Cochlea with Conclusions bearing on the Problem of Tone Perception. *Amer. Journal of Anatomy,* 1907, VII., 246–257.

MOORE and FRY. On the Appreciation of Difference of Phase of Sound-Waves. *Phil. Mag.,* XIII., No. 76, 1907.

W. V. D. BINGHAM. The Rôle of the Tympanic Mechanism in Audition. *Psychological Review,* XIV. (for July), 1907.

Several of the authorities here named give further references; see, *e. g.,* Kishi for literature bearing on the question of the nature of the basilar membrane. A. Kreidl, in his recent article, Die Function des Vestibular-apparates, *Ergebnisse der Physiologie,* 1907, gives a good bibliography on the subject which he treats, a subject which is of interest here.